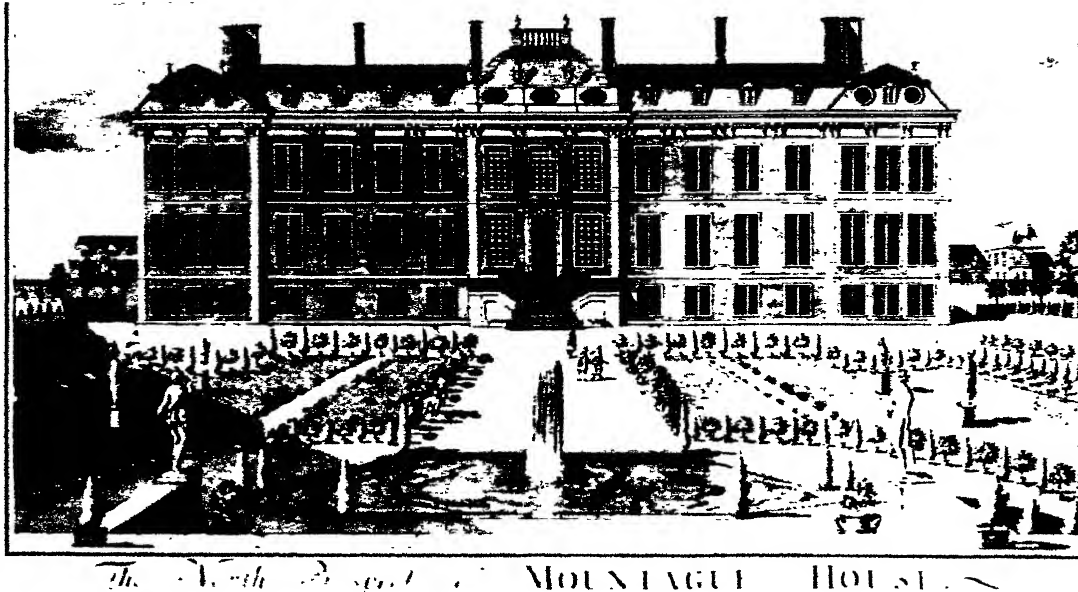
EARLY SCIENCE IN OXFORD

VII



MONTAGUE HOUSE, BUILT BY HOOKE IN 1676, AFTERWARDS USED AS THE FIRST HOME OF THE BRITISH MUSEUM. *See* p. 440

EARLY SCIENCE IN OXFORD

BY

R. T. GUNTHER

VOL. VII

THE LIFE AND WORK OF ROBERT HOOKE

(PART II)

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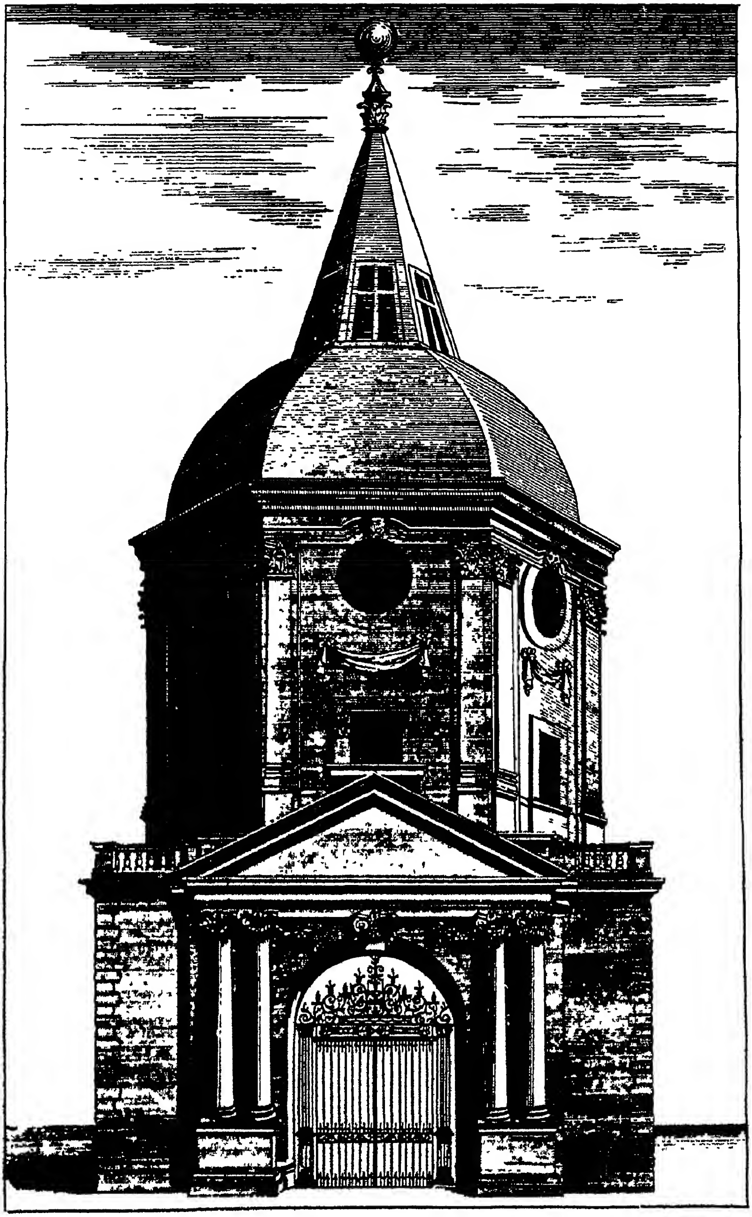
The present volume contains a summary of his work from 1672 to 1702, and an INDEX to the two volumes.

At the beginning is a note upon Hooke's share in the BUILDING OF THE COLLEGE OF PHYSICIANS.

LIST OF ILLUSTRATIONS

VOL. VII



THE COLLEGE OF PHYSICIANS 1673

From the Pharmacopoeia Londinensis 1721

into consideration and proceed in it according to the directing of the Act of Parliament for Intermixtures.

1671. *April* 6. Ordered also that Mr. HOOK treat with persons concerning the fellows houses and front houses, and give an account thereof to this Committee at the next meeting.

Junii 26. Concluded that Mr. HOOK the surveyor proceed and go on with the two houses on ye southside for the Chymist and Bedell.

1672. *Junii* 25. Ordered that Mr. HOOK have 20*li* pd him by the Treasurer for his late paynes and care about the Building of the College.

1672–3. *Feb.* 5. That the Fellows house in the College bee built uniform to the opposite side . . .

That Mr. HOOK bee intreated to present this proposall with the approbation of the Committee last appoynted for the Business.

That the street houses of the College bee determined by the said Committee at their next sitting, with Mr. HOOKE's advise.

1673. *Apr.* 3. Sir G. ENT proposed that seeing Mr. HOOKE'S Chapman fayles us, and no other makes a proposeall, to undertake it himself.

June 5. Mr. HOOK promised to bring in the scheame for the Theater within a week.

There is no reference to any work by WREN. The foundations for the new building were being laid on March 21, 1670–1, and during the course of the work regular payments were made to HOOKE as the following entries in the Cash book show:

1671. Oct. 19. To Mr. HOOK	£20	0	0
1672. July 2. To Mr. HOOK by ye College order	£20	0	0
1672–3. Jan. 16. To Mr. HOOK . . .	£20	0	0
1673. Nov. 7. To Mr. HOOK for his extraordnarie paines	£20	0	0
1674. Nov. 19. ,, by ye Presidents order	£20	0	0
1675. Oct. 13. ,, ...	£20	0	0

Thus there is documentary evidence that he prepared a 'scheame' for the Theatre and that he received fees amounting to £120. Only when the building was completed is there any mention of WREN, who apparently as architect to the King, and not in the capacity of architect of the College, accompanied a distinguished party of physicians on a visit to the new College.[1]

[1] See p. 422.

Soap-bubbles.

Mar. 28. Mr. HOOKE brought in his written account of an experiment, made March 13, upon a bubble of water and soap, which was ordered to be registered, and was as follows:

By the help of a small glass pipe there were blown several small bubbles out of a mixture of soap and water; where it was obvious to observe, that at the beginning to blow any of these bubbles, the orbicular film of water, which encompassed a globe of air, appeared white and clear, without any appearance of colour; but after some time the film by degrees growing thinner, (part thereof falling down, and part thereof evaporating and wasting into air) there appeared upon the surface thereof all variety of colours, that may be observed in a rainbow, beginning at first with a pale yellow, then orange, red, purple, blue, green, and so onward, with other the same series or successions of colours: in which it was further notable, that the first and last series of colours were very faint, and that the middlemost order or series was very bright and oriental. After these colours had passed over their several changes, the film of the bubble began to appear again white, and presently up and down in this second white film there appear several holes, which by degrees increase and grow bigger, and several of them break into one another, till at length they become very conspicuous and big. It is strange to observe, how those holes will, by the blowing or moving of the ambient air, be carried up and down upon the encompassed globe of air, and yet the bubble remain in its orbicular form without falling. It is yet further strange, that after this, when the bubble breaks, its breaking is with a kind of impetus or crack, dispersing the parts in a kind of powder or mist. It is yet further strange, that those parts of the bubble, which thus appear like holes through it, by the moving up and down upon the surface of the aerial globe will change its form, and from a circular be made elliptical, or any other undulated or waved form, in the same manner as any of the colours, that are visible on the bubble. It is yet more strange, that though it is most certain, that both the encompassing and encompassed air have surfaces, yet by no means, that I have yet made use of, will they afford either reflection or refraction, which all the other parts of the encompassed air do. It is pretty hard to imagine, what curious net or invisible body it is, that should keep the form of the bubble, or what kind of magnetism it is, that should keep the film of water from falling down, or the parts of included and including air from uniting. The experiment, though at first thought it may seem one of the most trivial in nature, yet as to

the finding out the nature and cause of reflection, refraction, colours, congruity and incongruity, and several other properties of nature, I look upon it as one of the most instructive: of which more hereafter perhaps.

Solution of Sal-nitre.

He made an experiment of dissolving sal-nitre in common water, whereby was caused a stream composed of water and of the particles of nitre dissolved therein; which stream was here descending, as in a former experiment a stream or fluid produced by a candle dissolved by the air ascended.

He was desired to bring in the description of it in writing.

New Object Glass.

Apr. 1. FLAMSTEED wrote to COLLINS, 'When Mr. SARGEANT comes to town I shall send money and directions for procuring me a new object glass, and beg your care, or Mr. HOOKE's in it'. . . . *Apr.* 17. 'Mr. HOOKE procured my last [object-glass] which was not very excellent. I hope he will not at your request refuse to try one when you speak for it, that I may not have a bad one. The two which he procured me cost 30*s.*; I shall send you 40*s.* . . . Pray impart to me what you know or hear of Mr. H.'s new tube.'

Experiment on Colours.

Apr. 4. Mr. HOOKE made an experiment with two pieces of glass stiffly rubbed upon one another, to show that there may be the same incidence of rays, and yet various colours. And he was ordered to bring an account of this experiment in writing at the next meeting, together with his considerations upon it; as also to draw up and give in an account of the experiment, made at the last meeting, with sal-nitre dissolved in common water.

Reflecting Prism.

Mr. OLDENBURG communicated Mr. NEWTON's proposal of a way of using, instead of the little oval metal in the reflecting telescope, a crystal figured like a triangular prism.

Mr. HOOKE was ordered to make such a crystalline prism for the design mentioned in Mr. NEWTON's letter, and to try the same.

Experiments on Colour.

Mr. HOOKE promised to bring in some other experiments of colours at the next meeting.

Apr. 9. J. GREGORY of St. Andrews wrote to COLLINS that he would gladly see what Mr. HOOKE can say against the doctrine raised upon the experiments of Mr. NEWTON on Colour.

Salary.

Apr. 10. It was ordered, that the Treasurer pay one year's salary to Mr. HOOKE.

Prisms.

Apr. 18. Mr. HOOKE was ready to make an experiment by a prism, viz. to destroy all colours by one prism, which had appeared before through another: but there being no sun, as was necessary, the experiment was deferred.

Steel concave Mirrors.

Mr. HOOKE proposed a way of making reflecting concaves of steel in great numbers, and polished by the means of two dies, one concave, the other convex, putting between them a plate of silver, and then stamping them with the mint-mill.

Prisms.

Apr. 24. Mr. HOOKE showed two experiments of colours with a couple of prisms. By the one it appeared, that one prism took off the colours, which the other had produced: by the other, that several colours were made by several refractions. He was ordered to give in the particular description of these experiments to be registered.

Steel Reflector.

May 8. Mr. COCK produced a piece of steel polished, to be used in the reflecting telescope.

Mr. HOOKE was desired to make trial with it, though he said it was falsely polished.

The Microscope.

Dr. GREW, the new curator for the anatomy of plants, being present, was desired to produce some observations on that subject at the next meeting; which he promised to do. In order to which it was ordered, that Mr. HOOKE should deliver to him the society's microscope.

Experiments on Light.

May 15. Mr. HOOKE made some experiments relating to Mr. NEWTON's theory of light and colours, which he was desired to bring in writing to be registered.

Prisms.

May 22. Mr. HOOKE made some more experiments with two prisms, confirming what Mr. NEWTON had said in his discourse on light and colours, viz. that rays of the light being separated

by one prism into distinct colours, the reflection made by another prism doth not alter those colours.

It was intimated by Mr. HOOKE, that these experiments were not cogent to prove, that light consists of different substances or divers powders, as it were; but that these phenomena might be explained by the motion of bodies propagated.

Newton's Letter.

June 11. Under this date NEWTON replied to HOOKE's attack on his discoveries concerning the nature of Light, Refractions and Colours, in the following letter addressed to OLDENBURG.[1]

Sir, I send you my answers to Mr. Hooke and P. Pardies, which I hope will bring with them that satisfaction which I promised. And as there is nothing in Mr. Hooke's considerations with which I am not well contented, so I presume there is as little in mine which he can except against, since you will easily see that I have industriously avoyded the intermixing of oblique and glancing expressions in my discourse. So that I hope it will be needless to trouble the R. Society to adjust matters. However, if there should possibly be any thing esteemed of that kind, I desire it may be interpreted candidly, and with respect to the contents of Mr. Hooke's considerations; and I shall readily give way to the mitigation of whatsoever the heads of the R. Society shall esteem personall. And, concerning my former answer to P. Pardies, I resigne to you the same liberty which he hath done for his objections, of mollifying any expressions that may have a shew of harshnesse.

Your Servant,

I. NEWTON.

[It will be remembered that Newton decided not to publish his *Optics* during the lifetime of Hooke. The matter is again mentioned in the NEWTON–OLDENBURG *Correspondence* published by Rigaud, p. 324, under dates June 19 and 25 and July 6.]

Experiments on Sunlight.

June 12. Mr. HOOKE was put in mind to bring in at the next meeting some experiments, that do not depend on the shining of the sun, together with those that require sunshine.

Refraction and Colour. The 'Black Spot'.

June 19. Mr. HOOKE's account of some experiments on refractions and colours, lately made by him before the society, was read, and ordered to be registered, as follows:

In order to examine, whether several colours, after the first

[1] Weld, Hist. R. S., p. 240.

refraction, would have several refractions, I made several experiments; and the first was, I took two prisms, and with the one I cast the rays of the sun upon a wall, at a considerable distance; by which means the several colours of the rainbow appeared in a line at right angles with that of the prism, viz. red, yellow, green, blue, and purple: then with a second prism, placed in a parallelism with the aforesaid line or order of colour, I a second time refracted the rays of the sun, or the several-coloured rays, and thereby found, that all the said colours would be a second time refracted, and yet keep their several colours distinct in the same order, without very sensibly intermingling any other colours with any of them. Moreover I found, that the several colours, though they kept their former order, viz. the second refracted red, yellow, green, blue, purple, kept the same order and position with the first refracted red, yellow, green, blue, purple; save only, that whereas I held the second prism in a parallelism with the order or line of colours, the colours cast upon the wall by this second refraction were not parallel, but askew, or oblique; and, upon examining which was most bent from the parallelism by refraction, I found the purple, and the rest less, in their order: that is, that the purple was farthest distant from the first line of refracted colours, and so consequently, according to the hypothesis of the differing refraction of colours, purple had the greatest refraction, blue the next less, green less than that, the next yellow, and red the least of all; which seems at first much to confirm Mr. NEWTON's theory of colours and light; but yet I think it not an *experimentum crucis*, as I may possibly show hereafter.

Next, I tried a second experiment, by casting with the first prism the line or order of colours upon the wall, and taking a black stick, and laying it exactly parallel with the said line, so as to touch the upper or under side of the said colours, if the colours were cast in a horizontal posture, or the right or left side, if cast in a perpendicular posture: then taking a second prism, and holding it exactly parallel with the stick or the line of colours, and looking through the same, I could plainly see the stick and the colours; and whereas, with my naked eye the colours seemed just to touch the stick, and to be parallel therewith, now they seemed to lie askew, and the stick to pass through them; and, which way soever the refraction of the second prism did bend the rays, that way did the purple bend most, and the red least; insomuch, that I have often observed, that when the colours of the first rainbow were cast above the stick, by looking on them thus, I could see them on the other; which at first seemed not a little strange.

Thirdly, I cast the colours by the first prism upon the floor,

or ceiling, or wall, and with the second prism held exactly, or, as near as may be, parallel with the first, I looked at those colours, but by such a side of the prism as refracted the rays quite contrary to their refractions in the first prism; and it was obvious to see, that by turning the second prism (whilst in its parallel position) round on its axis gently, the colours on the floor, wall, or ceiling, would by degrees quite vanish and disappear, and the rainbow (if I may so call the breadth of colours) which before possibly was almost a span breadth upon the place, contracted itself by degrees to the breadth of the first prism; but if the said rainbow was looked on through the second prism with a refraction the same way with the refraction of the first, the breadth of the same was very much stretched, and made a span and a half, or two spans, broad.

Fourthly, I took two thin pieces of glass well planed and polished, the thinner they are (so they do not break) the better; and putting them one upon another, I pressed them hard together till there began to appear a red coloured spot in the middle; then continuing to press them closer, I could plainly see several rainbows, (as I may so call them) of colours, encompassing the first plate; and continuing to press the same closer and closer, at last all the colours would disappear out of the middle of the circles, or rainbows, and the middle would appear white; and if yet I continued to press the said plates together, the white would in several places thereof turn into black. The first colour that appeared was red, then yellow, then green, then blue, then purple; then red, then yellow, green, blue, purple; red, yellow, green, blue, purple, and so onwards: so that I have numbered nine or ten several rainbows, or orders of colours, one immediately within another, and the red immediately next to the purple, and the last colour, that appeared before the white, was blue: so that it began with red and ended with purple, and where there was no other colour preceded, the red had no purple; but where red was on the one side, and blue on the other, there was purple; that way, that the red and the blue were dilated, was yellow, sky colour, and green; and that way they were heightened was purple: still the said rings, or rainbows, would vary their places, by varying the position of the eye, by which they were observed, and not only their positions, but their colours; so that the glasses remaining the same, that part, which was red in one posture of the eye, was blue to a second, green to a third, yellow to a fourth, and purple to a fifth, and other mixed colours to other postures. Moreover, that, which gives one colour by reflection, gives another by trajection, not much unlike the tincture of *lignum nephriticum*. Of the explication of these phenomena by various hypotheses more hereafter.

Experiments on Light.

Mr. HOOKE was desired to make more experiments of the same nature, for a further examination of Mr. NEWTON's doctrine of light and colours; especially such as might make it out, that colours may be varied by divers positions of the eye; as also those, that are made with plated bodies, showing, that the same inclination and the same thickness will give various colours: and further, those, that show in a dark room, that where there is no refraction nor reflection, there will be a succession of colours; so that the rays passing only a hole, colours will appear.

Speaking Trumpet.

June 26. Mr. HOOKE produced a new kind of speaking trumpet figured bellwise, repercussing and by degrees dilating itself; which being tried, and compared with that of Mr. CONYERS, was found to exceed it.

It was ordered, that against the next meeting, a pipe of the form of an ordinary trumpet should be made, widening by degrees in the form of a cone cut off, furnished with a mouth-piece.

Respiration.

Mr. TEMPLE's letter upon the internal structure of the lungs gave occasion to a discourse of respiration, and of the principal use thereof, which Mr. HOOKE said he thought to be, that by the air something essential to life might be conveyed into the blood; and something that was noisome to it, be discharged back into the air: and he wished, that means might be used to discover, whether there are not valves in the arteries, by which the air may pass into all the parts of the blood. For which purpose it was suggested, that an injection might be made into those vessels, and particularly into some artery, with melted beeswax, mixed with tallow, and coloured, thereby to make the vessels appear round and full, to see what might be further discovered in their structure. This was recommended to the physicians of the society.

Jupiter and his Satellites.

A paper by Signor CASSINI on the motion of Jupiter and his satellites was committed to the perusal and consideration of Mr. HOOKE, who was desired to make a report of it to the society at their next meeting, especially as the author expressed his desire of having the sense of the society, or some members thereof, upon the said paper.

July 3. Mr. HOOKE was called upon for making a report concerning Signor CASSINI's paper concerning the satellites of

Speculum Grinding.

Mr. HOOKE intimated, that the great tool for grinding the reflecting glass was now ready; and he was exhorted to put it to the trial, and to report the success to the society.

Discourse on Light.

Nov. 27. Mr. HOOKE produced and read a discourse of his own, containing divers optical trials made by himself, which seemed to discover some new properties of light, and to exhibit several phenomena, in his opinion not ascribable to reflection, or refraction, or any other till then known property of light.

He was desired to pursue these experiments in a convenient season, and to deliver in to the society some account of what was done on this subject, to be registered, to preserve his discoveries from being usurped.

Combustion.

He made an experiment to find out, whether air increases or decreases by burning: but the success not proving satisfactory, he was desired to repeat the experiment at the next meeting.

Speculum Grinding.

He being called upon concerning the large tool for grinding the reflex glass, said, that he had tried the said tool so far, as to find it pretty just.

Combustion.

Dec. 4. The experiment to find, whether air increases or decreases, being called for, Mr. HOOKE affirmed, that he had found, that it neither increased nor decreased. A trial of this being made before the society, it miscarried, and was therefore ordered to be repeated at the next meeting.

Light.

Mr. HOOKE being called upon about the giving in the heads of his late discourse concerning some new properties of light to be registered, promised, that upon further prosecution of that subject he would bring in the whole.

Speculum.

He gave hopes likewise, that he might be able to bring in the large reflex speculum at the next meeting.

Dec. 10.

LETTER FROM I. NEWTON AT CAMBRIDGE TO COLLINS

Sir. . . . As to the attempt, in which Mr. Rieve was employed,

I presumed it had been done with more accurateness than Mr. Gregory now signifies, because Mr. HOOKE, who you know is a curious and accurate experimenter, affirms in his considerations on my letter to Mr. Oldenburg, concerning refractions and colours, published in the Transactions, Numb. 80, that he made several experiments with that instrument. And though he lays the blame on Mr. Rieve's encheiria, yet he says not that he blamed him then, when the experiment was made. His words are these, 'I have made many trials both for telescopes and microscopes by reflexion, which I have mentioned in my Micrographia, but deserted it as to telescopes, when I considered that the focus of a spherical concave is not a point, but a line, and that the rays are less true reflected to a point by a concave than refracted by a convex, which made me seek that by refraction, which I found could not be expected from reflexion. Nor indeed could I find any effect of it by one of six feet radius, which about seven or eight years since Mr. Rieve made for Mr. Gregory, with which I made several trials; but it now appears that it was for want of a good encheiria, from which cause many good experiments have been lost. Both which considerations discouraged me from attempting further that way, especially since I found the parabola much more difficult to describe than the hyperbola or ellipsis.'

From hence I might well infer, that the want of a good encheiria appeared not till now, and that Mr. HOOKE was discouraged from attempting further that way only by these two or three considerations; that a convex (as he presumes) refracts more truly than a concave reflects; that he found no effect by one of six feet radius, which till now he attributed to some other cause than the want of a good encheiria, namely to the supposedly less true reflexion of a spherical concave; and he apprehended a greater difficulty of describing a parabola than an hyperbola or ellipsis. Nor could I well interpret the cause, from which many good experiments will have been lost, to have been other than the want of a good encheiria, which till afterwards appears not to have been wanting. I contend not that this was Mr. HOOKE's meaning, but only that his words seemed to import thus much; which gave me occasion to think there was no diligence wanting in making that experiment, especially since he expresseth that he made several trials with it.

your obliged humble servant,
NEWTON.

[Rigaud, *Correspondence*, pp. 345–6.]

No Experiments in Hooke's absence.

Dec. 18. Mr. HOOKE, the curator being absent, by reason of sickness, there were no experiments made at this meeting.

1672/3

Jan. 8. Mr. HOOKE was desired to prepare some experiments for the next meeting.

Speculum.

Jan. 22. Mr. HOOKE produced an essay of a reflecting objective speculum, being the segment of a sphere of thirty-six feet, which he hoped, when perfectly polished, would perform as much as a refracting object glass for a hundred-foot tube. He was desired to see it brought to perfection.

Jan. 29. Mr. HOOKE mentioned, that the reflecting speculum, which he had produced at the last meeting, was further published; and that he would endeavour to get it finished in a short time.

Feb. 5. Mr. HOOKE produced again his objective speculum for the reflecting telescope, which he affirmed to be now true, though not perfectly polished; which he would procure to be done against the next meeting.

Arithmetical Machine.

He mentioned, that he intended to have an arithmetical engine made, which should perform all the operations of arithmetic, with great expedition and certainty, without making use of the rhabdology, and that much more simply than that of Monsieur LEIBNITZ, produced before the society on the 22nd of January. He was encouraged to make good his proposition.

Weather-clock.

[A weather record kept at Paris with *Otto Guericke's Little Man*] gave occasion to speak of the weather-clock so often mentioned formerly, and so desirable and useful: and it was ordered, that Mr. HOOKE do not fail to get such a clock made as soon as possible; especially since in France, Italy, and Germany, the curious were known to be ready to join their observations on the weather to those made by the members of the society. Mr. HOOKE proposed to take care of this immediately.

Petrification in Bedfordshire.

Occasion being given to speak of petrifications, it was remarked by Mr. HOOKE, that he was credibly informed, that there was a ground in Bedfordshire, which would in a twelvemonth's time turn wood and other matter, that was not stony, into stone, without vitiating the figure.

Amber.

Mr. HOOKE declared his opinion that yellow amber was nothing but resin petrified.

Speculum.

It was ordered, that Mr. HOOKE'S objective speculum should be again produced and tried at the next meeting.

Musk Deer.

Feb. 19. The whole skin of a musk-deer was delivered to Mr. HOOKE for the repository.

Swammerdam's Treatise.

Mr. OLDENBURG delivered likewise to Mr. HOOKE Dr. SWAMMERDAM'S treatise, entitled, *Uteri muliebris Fabrica, una cum Methodo nova Cavitatis Corporis ita praeparandi, ut suam semper genuinam faciem servent*: printed at London, 1672, in 4to, and presented to the society by the author.

Combustion.

Mr. HOOKE tried again the experiment formerly attempted, of finding, whether air increases or decreases by burning: but it miscarrying again, he was desired to fit it better for the next meeting.

Mar. 5. Mr. HOOKE made an attempt again of the trial to find, whether air is generated or consumed by burning; but the apparatus failing again, he was ordered to fit it with care.

Arithmetical Engine.

He produced his arithmetical engine, mentioned by him in the meeting of February 5, and showed the manner of its operation, which was applauded. He was desired to bring in the description of it, that so it might the better appear how it differed from that of Monsieur LEIBNITZ, produced January 22, before the society.

Weather Clock.

Mr. HOOKE promised to give the society, at their next meeting, a lecture upon his weather-clock.

Mar. 12. Mr. HOOKE read a discourse of his upon the weather-clock; which being but a part of the whole intended by him, he promised to bring in the remainder, containing the description of the engine, at the next meeting.

He was desired to take care, that such an engine be made with speed.

Combustion.

Mar. 19. Mr. Hooke read a discourse of his, giving an account of the success of this experiment, which, he said, he had made, about the increase or diminution of air by burning; which was, that the air was diminished one-twentieth part.

He was desired to prosecute these experiments, and to give the society an account of them from time to time, and to bespeak some members of the society to assist at them.

Weather-clock and Speculum.

He was put in mind to prosecute the invention of the weather-clock, and to hasten the making of it; and not to forget the finishing of the reflecting speculum.

1673

Oil of Vitriol and Water.

Mar. 26. Mr. Hooke made an experiment of mingling oil of vitriol and common water together, thereby showing, that these two liquors were so incorporated, by entering into the pores of one another, that they took up less room, when mingled together, than they did both being apart. Of the common water there were twenty-one measures; of the oil of vitriol three measures, which is twenty-four measures in all, and yet mixed together they made but twenty-three measures.

Oil of Tartar and Aqua-fortis.

Apr. 2. Mr. Hooke made an experiment, by mixing oil of tartar and aqua-fortis together, to see how they would incorporate, and how much less space they would take up when thus incorporated than both apart. The mixture caused a great ebullition, which lasted all the while that the society sat: but it ran over several times, and therefore Mr. Hooke was desired to make it again.

Action of Aqua-fortis on Brass.

Apr. 9. Mr. Hooke made an experiment with aqua-fortis and a little piece of brass wire, put into that liquor, marking where the liquor stood before the putting in of the brass, and where, after it was put in: as also, how far it was raised upon its working upon the brass, and how low it descended afterwards, which was almost an inch below the mark, at which it stood at the first putting in of the brass.

He promised to bring in a full account in writing of this experiment, and of that made at the preceding meeting.

Incorporation of Liquors.

Apr. 16. Mr. HOOKE being called upon for an experiment, and having none ready, he was ordered to prosecute those lately begun about the incorporation of liquors, and to bring in a written account of those, that had been hitherto made.

Mr. HOOKE observed, that spirituous liquors, though well closed up, would in time commonly lose their virtue, and also their bulk, the particles of it passing into the pores of one another.

Precipitation.

May 7. Mr. HOOKE remarked, that it would be worth trying, what effect precipitation would have upon air by pouring certain liquors upon solutions to make precipitations.

Arithmetical Instruments and Rhabdology.

He read a paper of his concerning arithmetical instruments, as well those, that had been made upon the principle of the rhabdology, as that other shown to the society, January 22, by Monsieur LEIBNITZ; and he promised one of his own invention of better performance. This paper was ordered to be registered, as follows:

The best way for addition and subtraction is by setting down the numbers on paper, and proceeding as in common arithmetic; both these operations being quicker and much more certainly done than by any instrument whatsoever: for, first, the numbers may be writ down in half the time they can be set on any instrument; and, secondly, they remaining altogether in view, may be quickly added or subtracted, and the sum or remainder set down; and if there should be any mistake in the first, they can be presently run over again (which is not a quarter part of the trouble of the operation) whereas by an instrument to examine an operation over again, the whole trouble of the operation is performed; and a man is much more subject to miss in putting the key into the right number, than he is in setting down the figure to express it; and therefore, for those kinds of operations in arithmetic, an instrument is wholly insignificant, and at best will come short of common counters.

Next, the best instrument for squaring and cubing, or for extracting the square or cubic root, is by printed tables for that purpose, such as BABINGTON hath printed at the end of his fireworks, or Dr. PELL hath lately epitomized and reduced to a lesser volume: for by the help of printing a book of tables, which will presently resolve questions of that kind to twenty or thirty places, will be reduced into a less volume, and be purchased at a much cheaper rate than any arithmetic instrument, that shall do the whole operation itself without skill; and,

if skill in arithmetic be allowed to be joined in the use of the instrument, printing will furnish us with tables, that will do all those kind of operations with much more certainty and speed, than any arithmetical instrument yet known.

Thirdly, as to multiplication and division, the Lord NEPER, in his *Rhabdology*, hath taught a very excellent facilitating method, by the help of small rods, which I take to be the plainest, shortest, and exactest method of using that help, much better than that of Monsieur PETIT, of putting them on a cylinder, or any other way of putting them on movable wheels; that way taking up much less room, being more easily changed and varied, and being capable of the advantage of the press, which makes them much less chargeable and cumbersome, for they may be printed on parchment, and cut into strips, which may be afforded very cheap, will take up very little room, and they may be made use of to what number of places one will.

Or, if one will avoid setting down the intermediate products, his compound rhabdology may be made use of, by printing those roads, or places, on parchment, both for the figures and holes, by the help of which there is no use of addition or setting down till last of all, or that the whole operation be completed.

As for the arithmetical instrument, the model of which was produced here before this society, it seemed to me so complicated with wheels, pinions, cantrights, springs, screws, stops, and truckles, that I could not perceive it ever to be of any great use, especially common use: first, because the multitude of the parts must vastly augment the charge and bulk thereof; so that it could only be fit for great persons to purchase, and for great force to remove and manage, and for great wits to understand and comprehend: secondly, because the multitude of its parts must make it exceeding hard to be put into good order, and extraordinary apt to be put out of it; besides, I saw no means of examining, whether the operation had been truly performed, without trying it over again, which is intolerable. The design, indeed, is very good, which is the only thing I was able to understand of it, which is to give the product and quotient of a multiplication or division, which Sir SAMUEL MORLAND's instrument is not at all adapted to. But I have an instrument now making, which will perform the same effects with the German, which will not have a tenth part of the number of parts, and not take up a twentieth part of the room, that shall perform all the operations with the greatest ease and certainty imaginable; whereby in large numbers, for multiplication or division, one man may be able to do more than twenty by the common way of working arithmetic, and, that without at all troubling his memory or ratiocination, and this by two instruments quite

differing in their principle and contrivances; the description of which I design to present to this honourable society, after the model promised by Monsieur LEIBNITZ to be sent from Paris to this society to be here seen and examined.

Sir S. Morland's Book Purchased.

Mr. HOOKE desired, that Sir SAMUEL MORLAND's book, entitled, *the Description and Use of two arithmetic Instruments,* &c. printed at London, 1673, 12mo, might be purchased for the society's library; which was ordered to be done.

Combustion.

May 14. Mr. HOOKE made an experiment with the air, produced April 23, in a bladder by the operation of aqua-fortis upon oyster shells, having first tried how long a slender white wax candle would burn with common air, which it did, in one glass, during the space of sometimes seventeen, sometimes twenty or twenty-one vibrations of a pendulum of about a second; in another bigger glass, during the space of 55 vibrations: whereas the factitious air, being by a certain contrivance squeezed out into the larger glass, yet so that some of the common air remained in it, the said wax candle burnt in it only forty-five such vibrations.

This experiment being not accurate enough, Mr. HOOKE was desired to make another apparatus for a better trial.

June 4. Mr. HOOKE made an experiment with air produced out of bottled ale, putting it into a glass vessel, in order to see, whether, and how long, a candle would burn in it; and it was found, that it would no more burn in this air, than it did in air generated out of aqua-fortis and pounded oyster shells.

It was proposed, that something might be thought upon for correcting this air, so as to make a candle burn or animals live in it.

Mr. HOOKE said, that he would consider of it, and try, whether it might be corrected by precipitation.

Weighing Anchors.

Sir ROBERT MORAY related that Sir SAMUEL MORLAND had proposed a method of weighing anchors with ease and safety. Whereupon Mr. HOOKE affirmed, that he had several years ago invented a convenient method of doing the same thing; which having discoursed of somewhat in general, he was desired to acquaint the society with the particulars at another meeting.

College of Physicians.

June 5. Scheme for the Theatre. *See* p. viii.

Weather-clock.

Oct. 30. Mr. HOOKE being called upon for experiments, and particularly concerning the weather-clock, said, that he would prepare some experiments for the next week, and take care of having the weather-clock made; as also of finishing his discourse upon it.

Water Spreading.

Nov. 6. Mr. HOOKE showed an experiment of water spreading itself, by a peculiar contrivance of a pipe, into a canopy (not a parabola, as is the ordinary way) and reverting into the perpendicular, whence it came. Which figure he ascribed to the water's tenacity.

College of Physicians.

Nov. 7. HOOKE received his fourth fee of £20 'for his extraordnarie paines' over this new building.

Nov. 13. Mr. HOOKE was called upon for his account of the experiment made November 6, of water spreading itself into a canopy, and reverting to the perpendicular. He excusing himself, that he had not been able to make it ready, was desired to prepare it for the next meeting'.

Elasticity of Glass.

He showed an experiment concerning the springiness of glass, by applying to a slender glass pipe a wax-light on all the sides thereof, by which it appeared, that the light being held on the top of the pipe, the farther end of the pipe sunk; held underneath, it rose; held on the side towards the hand applying, it turned from him; held on the opposite side, it turned towards him.

The cause of this phenomenon was by some conceived to be the expansion of the glass on that side, where the light is applied.

Sir WILLIAM PETTY said, that it was a desirable thing to have a good theory of the springiness in bodies.

Mr. HOOKE mentioned, that formerly he had explained it, in a discourse of his, brought in upon the occasion of the odd phenomenon of the pipe of mercury standing top-full far above the ordinary station.

He was desired, since that discourse was not yet brought in by him, that he would bring it in; which he promised to do.

He promised also to bring in some experiment or other at the next meeting.

Microscope.

Nov. 20. Mr. HOOKE showed a microscope, with one only globule of glass, fastened to an instrument with many joints, to turn every way, and so to show the object on every side with

greater distinctness than other microscopes: which kind of microscope, he said, a German had brought over with him out of Holland, but that it had been long since hinted by himself in the preface to his *Micrographia*.

Water Spreading and Elasticity.

He was put in mind both of his account of the experiment made November 6, with water, and of his discourse concerning elasticity.

Vacuum.

Nov. 27. Mr. HOOKE showed an attempt of his, of making a vessel so thin, that when evacuated of the air contained in it, it might swim in the air. He mentioned also, that a certain Italian clergyman, named LANA, had written upon this subject; whose book he thought had been formerly presented to the society by their secretary, but was still in his hands.

Elasticity of Coal.

Dec. 4. Mr. HOOKE showed an experiment of the springiness of coal; which was, that one side of a piece of charred wood or coal being heated, that side did (as in the like experiment formerly made with glass) bend from the heat, as appeared by a long stick fastened thereto, and the end pointing to a fixed mark.

It was also tried again with a glass pipe, as likewise with a brass wire; which latter stirred but very little, and almost insensibly.

Smethwick's Microscope.

There was produced a microscope of Mr. SMETHWICKE's contrivance, said by him, as Mr. HOOKE reported, to have glasses not spherical, but of a conic section: which figure the author, as was said, affirmed he could make and polish with certainty. Being tried, some of the members found it show the object very distinctly without any colours, and magnify it very considerably.

It was thought necessary to compare it with some very good ones of a spherical figure.

Attraction of Loadstone.

Dec. 11. Mr. HOOKE brought in an apparatus to show by experiments the strength of the loadstone's attraction, and to find in what proportion it draws, at several distances.

He was ordered to fit this apparatus so, that the design of it might be well prosecuted.

Dec. 18. Mr. HOOKE produced again his instrument for determining the force of the loadstone's attraction at certain distances: but the apparatus still failing, he was desired to fit it better for the next meeting.

Considerable Experiments.

Dec. 22. It was ordered, that Mr. BOYLE, Sir WILLIAM PETTY, Sir CHRISTOPHER WREN, Dr. GODDARD, Dr. GREW, and Mr. HOOKE, be desired to draw up a list of considerable experiments to be tried before the society, and to prepare an apparatus necessary for the exhibition of them upon all occasions.

Latten Works.

Dec. 26. 'Mr. YARRINGTON, who had seen the Latten-making Works near Leipzig, said many plates are beaten under the hammer at once, like Leaf-Gold or Tinfoil. The great difficulty is how to turn them under the hammer quick enough.'

[Hooke's *Diary*, quoted by Derham, p. 186.]

1673/4

Limit to graduation of a Scale.

Jan. 15. Mr. HOOKE made an experiment with a ruler divided into such parts, as being placed at a certain distance from the eye, appeared to subtend a minute of a degree; and being earnestly and curiously viewed by all the persons present, it appeared, that not any one present, being placed at the assigned distance, was able to distinguish those parts, which appeared of the bigness of a minute, but that they appeared confused. This experiment he produced, in order to show, that we cannot by the naked eye make any astronomical or other observation to a greater exactness than that of a minute, by reason that whatever object appears under a less angle, is not distinguishable by the naked eye; and therefore he alleged, that whatever curiosity was used to make the divisions of an instrument more nice, was of no use, unless the eye were assisted by other helps from optic glasses.

Improvement in accuracy of an Astronomical Quadrant.

Jan. 22. Mr. HOOKE proposed the making of a new kind of astronomical instrument of his own invention for the taking of heights, angles, and distances, of celestial bodies by one observation more exactly than ever was yet done, viz. to a second. He added, that in this way the exactness of the instrument, as to divisions, sights, and perpendicularity, might, upon occasion, be duly ordered by the astronomical observer, so as not to rely upon the credit or skill of the instrument-maker.

He being asked, what the making of such a quadrant would amount to, and answering, that he thought it could be made for less than ten pounds, it was ordered, that he should cause one to be made of that price.

Minutes Indistinguishable.

The experiment made at the last meeting, to show, that with common sights we are not capable to distinguish a minute, was repeated; and proved what it was designed for.

The First 'Gregorian' Reflecting Telescope.

Feb. 5. Mr. HOOKE produced a new kind of reflecting telescope of his own contrivance, differing from that of Mr. NEWTON in this, that the observer looked directly at the object erected. This was performed by a way propounded by MERSENNUS, and repeated in Mr. GREGORY'S *Optics*; but was thought to have been never actually done before.

Discourse on Stars.

Feb. 12. It was ordered, that a discourse of Mr. ROBERT HOOKE about the stars be printed by JOHN MARTYN, printer of the Royal Society.

Loadstone.

Mr. HOOKE made several trials with a loadstone, to find, whether the interposition of any body would hinder the power of its effluvia: and having by weight so poised a balance, fitted for this purpose, that the iron was made to hang at a certain distance from the loadstone, so as to leave some room for divers bodies to be interposed, it was found, that though a silver crown, a piece of glass of about the same thickness, and four twenty-shilling pieces of gold, called guineas, were severally interposed between the iron and the magnet, yet the iron did not at all alter its distance, which at first it had by the poise.

It was ordered, that Mr. HOOKE should be desired to try by himself a good number of experiments upon this subject, and draw up an account of their success, and to communicate it to the society, that so they might call for such of them as they should think good to be shown before them.

Magnet.

Feb. 19. There were made some more experiments with the magnet; viz. a small bar of steel, of about one inch in length, and about a quarter of an inch diameter, was so suspended, that the lower end was distant from the pole of a very good loadstone, and counterpoised by six grains. And by several trials it was found, that a plate of glass, six inches broad, and half an inch thick, a thin board of wood about the same bulk, a plate of spar about the same breadth and thickness, a cut of butter on a trencher, a pewter plate, a set of brass weights, a burning deal board, a red-hot tile, a bright-burning coal, the same also blowed,

each of them interposed between the steel and loadstone, suspended as aforesaid, made no variation of the attractive virtue of the stone: nor did an onion, slit in two, and laid upon the same stone, nor the interposing of lead, glass, wood, silver and gold, all at once, make any manner of change, the attraction remaining constant.

Further, by the interposing of a knife, the virtue of the loadstone was much diminished; but by interposing a little bar of iron, half an inch in length, endwise, it increased about a quarter of the strength.

The bringing of iron anywise near the stone weakened the attraction.

Four-inch Quadrant.

Mr. Hooke produced a quadrant of four inches diameter, with telescopical sights, to be made use of by two observers, and distinguishing to minutes.

Reflecting Instrument.

He was put in mind of his other quadrant, whereby parallaxes, refractions, &c. may be observed in seconds by one observer.

Terrella.

Feb. 25. Mr. Hooke showed an experiment of the inclination of the lines of direction to the axis of the terrella; which he performed by placing a terrella in an hemispherical hole cut in a round table, and ordering the terrella so, that the axis lay level with the surface of it. Upon the surface of this table was placed a large skin of parchment, stretched on a hoop like a drumhead, in the middle of which was cut a circular hole, just big enough to receive the terrella. Upon this parchment were sifted fine filings of iron, which by the gentle vibration of the extended parchment soon ranged themselves into magnetical orbs, which were thought to be all of an oval figure, and of ovals of one kind, but of different bigness, and all of them to touch the axis in the centre of the loadstone. But these being only conjectures, and not certainly verified, it was thought proper, in order to the clearer and more certain discovery thereof, that there should be other methods attempted to make it out; which Mr. Hooke propounded, and engaged to have at least some of them ready against the next meeting.

He observed further, that a loadstone being moved to and fro under the parchment, on which the filings lay scattered, those filings all rose up, like so many bristles, making an appearance, as if the loadstone had been seen through the parchment.

He likewise applying a loadstone close to a small piece of tin,

the stone seemed to hold it both after it was rubbed, and without rubbing.

He suggested also the making of experiments with a capped and uncapped loadstone, interposing a single paper between it and a piece of tin.

He was desired also to show some of these experiments at the next meeting.

Catalogue of Arundel Library.

Feb. 27. It was ordered, that Mr. Hooke take care of having the catalogue of the Arundelian library completed within a month, and to have a duplicate made thereof.

Magnetical Experiments.

Mar. 5. The President and all the Vice-Presidents being absent, the society did not sit; yet Mr. Hooke repeated the magnetical experiment, which had been made at the last meeting, and which seemed to confirm that phenomenon of the magnetical orbs ranging themselves into elliptical figures.

There were also made some experiments with a loadstone, capped and uncapped, viz.:

The south end of an uncapped magnet, with a single paper between it, and a small bar of steel suspended, held the said bar with four drachms and fifteen grains.

The same south end capped, without paper, held the said bar with seven ounces and one drachm.

The same south end capped, with a single paper interposed, held that bar, with one ounce, five drachms, and an half.

Mar. 12. Mr. Hooke endeavoured to show a new way of making a loadstone set itself north and south; which was, by suspending it by a string fastened to the two ears of a cap, like the cap of a magnetical needle, which was convertible upon the sharp point of a needle. But the contrivance not proving tender enough, he was desired to fit it better for the next meeting.

He intimated a theory for finding the loadstone's variations all over the world, and promised to make an apparatus for it against the next meeting, and particularly a rete for the magnetical meridian.

The experiment about the inclination of the magnetical direction to the axis of the terrella being again made, and those lines considered, several of the members doubted whether they were ovals of one kind. Mr. Hooke was therefore desired to contrive some method of determining this point.

Magnetic Variation.

Mar. 19. Mr. Hooke gave an account of a theory for finding

the variation of the magnetic needle all over the world; of which he said that he knew not, whether it was coincident with that of Mr. Henry Bond, who many years before had pretended to know such a theory; whence he could likewise deduce the longitude.

The substance of Mr. Hooke's theory is, that the magnet hath its peculiar pole, distant ten degrees from the pole of the earth, about which it moves; so as to make a revolution in three hundred and seventy years: whence the variation hath altered of late about ten or eleven minutes every year, and will probably so continue to do for some time, till it begins to grow slower and slower, and will at length be stationary and retrograde, and in probability may return. But whether it be so or not, or whether it proceeds in a meridian, or in a parallel or great circle, or any other irregular curve, and if in a curve, whether its concave or convex sides be towards us, more time and observations must make clear. But it seems most probable, by comparing several declinations, observed by Captain James and others, that the progress of this magnetical north pole is from west to east beyond the north pole.

Mr. Hooke proposed the making of an easy and nice instrument, for observing exactly the variations of the needle in many different parts of the world; and he was desired to procure it to be made.

1674

Magnetometer.

Mar. 26. Mr. Hooke repeated his discourse made at the last meeting, concerning an hypothesis for solving the phenomena of all the variations of the magnetical needle all over the world; as also his purpose of preparing an easy and accurate way, to be sent abroad, for making exact magnetical observations.

Meridian at Gresham College.

He was desired to begin himself, by making good observations of the needle's variation here; and, in order thereunto, to fix a certain meridian at Gresham College; which he undertook to do by the north star.

Under date March 31, '74, Oldenburg wrote to Lister: "I suppose Mr. Martin hath, ere this, sent into your quarters some copies of Mr. Hook's printed attempt for proving ye Motion of ye Earth from observations made by himself, manifesting yt there is a sensible parallax of ye Earths orb among ye fixt stars; which if there be no default in them will amount to a demonstration of ye Copernican system agst that of Ptolemy and Tycho. . . . Pray, Sir, oblige me to acquaint Mr. Brook & Mr. Sisson herewith . . . ."

Magnetical Experiments.

Apr. 2. Mr. Hooke showed some experiments, concerning the various ways, that the magnetical effluvia bend and inflect

themselves, by putting divers straight steel bars in the pole of the magnet, some shorter, some longer, some close to it, some at a distance, some in direction, some crosswise; the effect whereof was, that the filings of iron being stirred by gentle knockings of the extended parchment, ranged themselves into oval or curve figures about the bars, but in a quite different form from what they would have received, had those bars of iron been loadstones of the like shape; that is, the poles seemed to lie in these, where the equinoctial would have been in a magnet, and the equinoctials of these would have been the poles of loadstones of like shape.

He promised to prosecute these experiments, by applying bodies of iron of other figures to the terrella.

He mentioned also, that whereas a loadstone would attract a red-hot iron, steel would not be at all affected by it.

Microscopic Appearance of Bullrush.

Apr. 23. Mr. HOOKE showed by a microscope the inward texture of a bullrush, consisting of pipes interwoven from one end to the other, in the manner of a hurdle, or resembling loose needlework.

He made an experiment for discovering, whether a bar of steel, touched by the immediate contact of a loadstone would more strongly move to it, than untouched. It was found, that the steel suspended at a balance, and counterpoised, when thus touched, bore the same weight when untouched.

Quadrant with Telescopic Sights.

He likewise produced a quadrant with telescopical sights, wherein appeared the pre-eminence of such sights above the common dioptra's.

Parallax of Sun.

HEVELIUS having written that the French astronomers had observed the sun to have no parallax at all, Mr. HOOKE said, that it was desirable to know by what method the French astronomers had made that observation of the sun having no parallax at all; that he was persuaded, that if the observations were made with telescopical sights, some, though a very small, parallax of the sun would be found; and that by the naked eye, be the instruments never so accurate, one cannot observe to less than a minute; whereas that parallax will scarce perhaps amount to a quarter of a minute.

Reflecting Quadrant.

Apr. 30. Mr. HOOKE excused himself, that his quadrant formerly promised was not yet ready.

Terrella and Ring.

He made an experiment, whether an iron ring would, by any magnetical virtue, be kept in a posture encompassing the terella at equal distance. And it was found, upon making several essays with the said ring, that at length it rested about the terrella unmoved, lying upon a board in water. This was tried, to see whether anything could be found here below analogous to the circle about the planet Saturn.

It was moved, that experiments might be made, to find:

1. Whether all parts of the terrella have an attraction directly towards its centre?

2. Whether, if there be any such attraction, that attraction be in all places of the terrella of equal strength; for instance, in the equator as strong as in the axis?

3. Having by trials found, what the approaches of magnetical bodies to the magnet are, according to the different position of the magnet, perpendicular, horizontal, and oblique; to endeavour to find out, since the approaches are made in a curve line, what kind of curve it is?

Dip Needles.

Mr. Hooke suggested, that the best dipping-needles may be made in water, because the water takes off the gravity; as also, that a pipe of iron should be made of equal gravity with water dipping.

College of Physicians.

May 13. Mox sancitum est ut Dr. Glisson, Dr. Goddard, Dr. Whistler et Dnus Carolus Scarburgh Aedilem Regium Christopherum Wren et Mrum Hooke in Collegio conveniant; vis uri quinam locus extruendo Theatro maxime idoneus sit. (Munk's *Roll.*)

May 26.

(22) *An attempt to prove the Motion of the Earth from Observations.* pp. [viii]+28, 1 pl.

Cutlerian Lectures, No. 1, dated March 25, 1674.
Notice in *Phil. Trans.*, No. 101, p. 10, March 1674.

EXPLANATION OF PLATE.

Fig. 1. S = Sun in centre of Planetary Orbs A B C D; . . . $\alpha\,\beta\,\gamma\,\delta$; ♈ ♋ ♎ ♑ = orb of Earth; 12 = axis of Earth; D = a star; C = Zenith of Gresham College at noon when Earth is in Cancer; A = Zenith of ditto at midnight when Earth is in Capricorn.

Fig. 2. AB diameter of great orb; AC and AD perpendiculars of

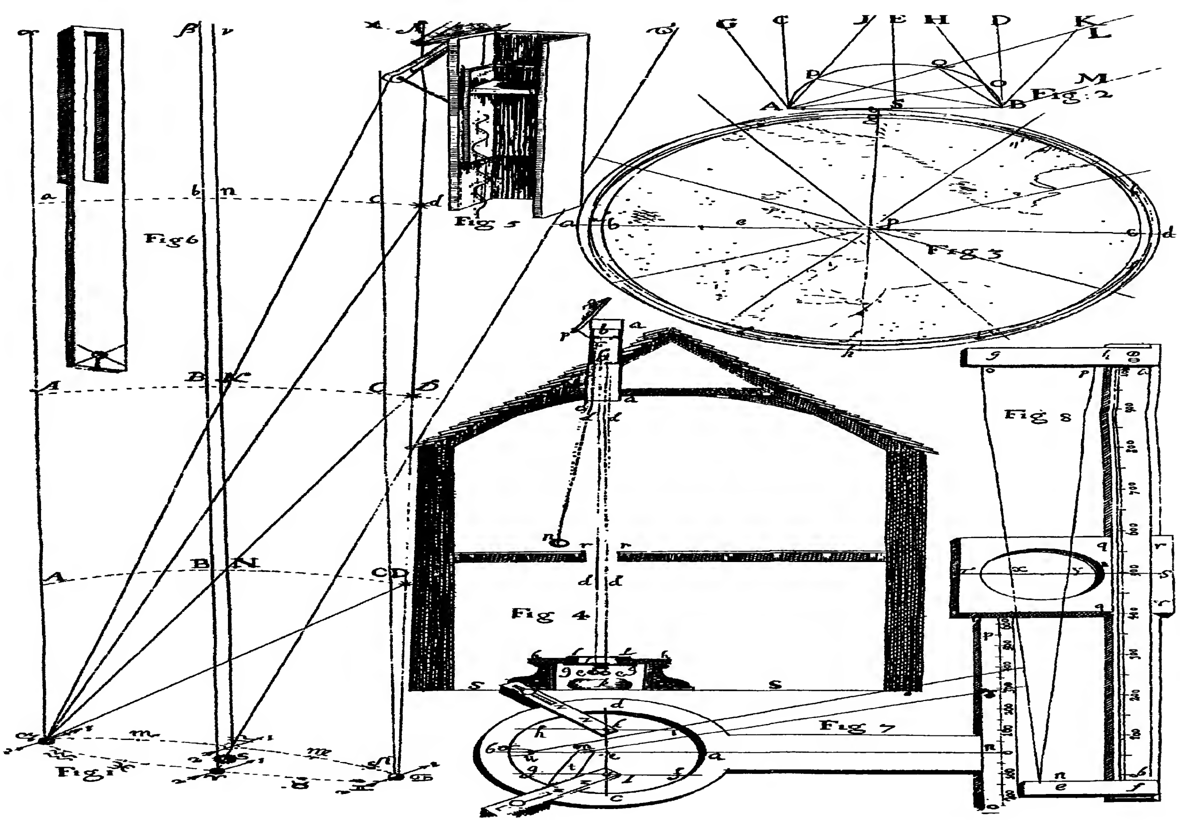


Fig 1
Fig 2
Fig 3
Fig 4
Fig 5
Fig 6
Fig 7
Fig 8

Iceland; GA and HB perpendiculars of Gresham College in Draco; LA and MB ditto to the solstitial colure near Auriga.

FIG. 3. Stereographical projection of circumpolar stars on a plane perpendicular to the axis of the Earth.

p = Pole; *e* = pole of the ecliptic; *l* = Star in head of Draco; *accc, bddd, efff, ghhh* = imaginary circles described by Zenith of Gresham College among fixt stars in June, December, March, and September respectively.

FIG. 4. Section through zenith telescope in Hooke's Lodgings at Gresham College. *aa* perpendicular tube 10 ft. long by 1 ft. square through ceiling, covered by a lid *p* opened by string *nop*. A smaller sliding tube *bb* with cell *c* for object glass of 36 ft. focus. From N. and S. sides of *c* depend two plumblines *dd*, passing through first floor *rr* to lower floor *ss*, on which is a table *ihhi*. Under table top is a slider *gg* with eye glass *f*.

FIG. 5. Enlarged view of zenith telescope tubes in roof of Gresham College.

FIG. 6. Enlarged view of steady bar shown as *zz* in Fig. 7.

FIG. 7. Brass Mensurator for measuring distance of stars from zenith. With 9 inch aperture *ab* crossed by hairs *ab* and *cd* showing zenith *ae*. Two other hairs *fg* and *ih*, parallel to the East West line cut the meridian at *k* and *l*. To the end of bar *en*, which is 15 times the length of *em*, is fixed a 3½ foot divided rule *op*, by which seconds of a degree could be measured, with the aid of the eye glass. *zz* steady bars to check swing of plumb lines.

FIG. 8. Alternative form of Mensurator. *ab* 3 or 4 ft. rule divided into 1,000 or 10,000 equal parts. On and from two fine threads starting from *o* and *p* one inch apart on two brackets *gh*, crossing at *n* or *ef* bracket. *st* cross hair strained across moveable slider *qr* cutting threads in *x* and *y*. 'Any workman will be able to make it.'

Reflecting Quadrant.

June 4. Mr. HOOKE being called upon for his new astronomical quadrant, said, that he hoped, that it would be finished very soon: and being desired to acquaint the society with the performances to be expected from this instrument, he answered:

That it was a quadrant so contrived, as to perform what could be required from any astronomical instrument; the particulars whereof he intended shortly to publish in print. He was desired to hasten the finishing of so noble and so useful an instrument; and to get it ready, if possible, against the next meeting.

Magnetic Experiments.

He was put in mind of preparing such experiments as might

determine those particulars, which were suggested by him April 30, 1674.

Wilkins's Legacy.

Oct. 7. It was ordered, that Sir JOHN LOWTHER, Sir WILLIAM PETTY, and Mr. HOOKE do meet together, and consider of a safe and beneficial way of putting out the four hundred pounds, left by the late Dr. WILKINS, Bishop of Chester.

Sir Jonas Moore and Chelsea College.

Oct. 19. Mr. HOOKE acquainted the council, that Sir JONAS MOORE had been with him at Chelsea College, and made an overture of engaging a gardener, a sufficient man, to take a lease of the house and land about it, for a considerable number of years, on condition of repairing the house and wall in the land, and paying a yearly rent for it; allowing withal to the society a power to make hortulan experiments there; as also to build an astronomical observatory; which latter Sir JONAS MOORE himself would undertake to do at his own charges, to the value of a hundred and fifty or two hundred pounds.

This proposition was well accepted by the council, and Mr. HOOKE was desired to prosecute the business, by urging Sir JONAS MOORE to proceed further in this affair.

Wilkins's Legacy.

The legacy of the four hundred pounds being also again considered of, and Sir WILLIAM PETTY having made an overture of laying out that sum upon a house of the late Captain GRAUNT in Birchen Lane, the council desired, that Sir JOHN LOWTHER, Sir WILLIAM PETTY, and Mr. HOOKE would meet together and ripen that business.

Animadversions on the Machina Coelestis.

Nov. 9. Mr. HOOKE's discourse containing *Animadversions on the first part of* Machina Coelestis *of the deservedly famous astronomer,* JOHN HEVELIUS, &c. was licensed for the press.

Hog Lane Houses.

Mr. HOOKE was desired against the next meeting of the council to view the place in Hog Lane, whether the houses were in good repair, and likely to be tenanted.

Nov. 12. Memorandum. To inquire of Mr. HOOKE, whether he had viewed the house in Hog Lane.

Elasticity.

It being, among other particulars in discourse upon the reading of Dr. WALLIS's paper, remarked, that the explication of the cause of springiness would contribute very much to illustrate

the nature of air, Mr. HOOKE said, that he had considered that subject, and particularly to make a springy body out of a body not springy.

Discourse Promised.

Mr. HOOKE was desired by the President, that if he should perform what he mentioned, he would present the society therewith in a discourse: and being asked, whether he could promise to bring it in at the first meeting of the society after the approaching holidays, he answered, that he would endeavour to do so.

Hog Lane Houses.

Nov. 19. Mr. HOOKE was again desired to view the houses in Hog Lane proposed by Sir WILLIAM PETTY for laying out the four hundred pounds legacy upon, and to make a report to the council at their next meeting.

Sir WILLIAM PETTY, Mr. HOSKYNS, and Mr. HOOKE, were ordered to take care of the proposal for disposing of the four hundred pounds legacy upon the houses and land in Hog Lane.

Nov. 23. Mr. HOOKE being called upon to make a report of the view, which he had been desired to make, of the houses and lands in Hog Lane, proposed by Sir WILLIAM PETTY for employing the four hundred pounds legacy; and he not having yet taken that view, was desired again to do it against the next meeting of the council.

Nov. 26. Mr. HOOKE gave the council some account of the houses and lands in Hog Lane, proposed by Sir WILLIAM PETTY, the consideration whereof was referred to another meeting.

An Experimental Discourse or a Penalty.

Dec. 3. It was resolved, that every member of the present council shall provide an experimental discourse for the society to be made at some one public meeting within the year, either by himself or by some other member of the society; or to pay forty shillings.

Subscription.

It was ordered, that Mr. OLDENBURG be desired to offer the new legal obligation for paying fifty-two shillings a year for the use of the Royal Society, to as many members of the same to sign and seal, as conveniently he can; and likewise show them the statute made by the council to engage every fellow of the society to such a subscription:

Catalogues.

That there be forthwith made a catalogue of all the presents

made by several persons to the society; together with the names of the donors; and that duplicates thereof be made, the one to be kept by the keeper of the repository, and the other by the Treasurer *pro tempore*:

That a catalogue be made of all the instruments or other apparatus of the society, paid for out of the public treasury; and that the instruments be looked out and kept together in the repository for instruments:

That a table and catalogue be made of all the books, discourses, letters, and accounts, brought into the society; together with the names of the authors: and that all the said books, discourses, letters, and accounts, be kept in convenient presses under locks and keys, and that the President and Secretaries *pro tempore* have the keeping of the said keys.

Reflecting Quadrant.

Mr. HOOKE read his discourse concerning the construction and uses of his new quadrant for making remote observations with great exactness.

Experiments.

Dec. 17. Dr. Cox having promised to entertain the society on January 7, Mr. HOOKE undertook to do the like on the 14th of that month.

Mr. HOOKE having proposed to the council, that in order to the bringing in of several sets of experiments, that would require an apparatus of instruments for the making of them, such instruments might be ordered to be prepared, whilst he was drawing up such experiments; the council resolved, that the sets of experiments should be first brought in before them; and that then they would consider of and give order for such instruments, as should be necessary for exhibiting the same.

Magnetical Experiments.

The council further desiring Mr. HOOKE to name a set of experiments to begin with, he named those of the magnet.

Horizontal Sails.

Mr. HOOKE produced and explained his model for horizontal sails, being persuaded, that he had improved that position of sails to the greatest perfection, of which it was capable; since those sails could not, in his opinion, be put in any posture more advantageous than that, which he exhibited.

Gresham Observatory.

Dec. 19. For a turret over the apartments of the Professor of

Geometry for astronomical purposes the Gresham Committee voted £40, and HOOKE availed himself of the facilities afforded him for carrying on several astronomical and meteorological observations.

1674/5

Eclipse of Moon.

Jan. 14. Mr. HOOKE read his observations of the late lunar eclipse of January 1, at which were present the President, Sir JONAS MOORE, and Mr. COLLINS. He was desired to perfect this discourse, and to publish it.

Nature of Trees.

Jan. 21. It [Dr. DANIEL COXE's lecture concerning the analysis of vegetables] gave occasion to debate Mr. HOOKE's notion of the nature of trees, viz. that it consists in the dissolution of bodies by air.

Elasticity.

Mr. HOOKE intimated also upon occasion, that he hoped he should be able to make it out, that a body may be made springy out of particles, that have no spring.

He was desired to endeavour to prove this by experiment as soon as he could.

Helioscope.

Jan. 28. Mr. HOOKE read his discourse concerning his new contrivance of a helioscope and divers other useful instruments.[1] The helioscope was for observing the sun without offending the tenderest eye by the help of several reflecting glasses weakening the strokes of the sunbeams.

It was ordered, that the helioscope should be fixed and tried on the first sunshiny day.

Microscopic Structure of Muscle.

Feb. 4. Upon the occasion given by a discourse by Dr. KING concerning muscular motion Mr. HOOKE declared, that he had made some discovery of the structure of a muscle by inspection with a microscope.

Dr. CROONE supposed the fleshy parallelopipeds [of muscle] to consist of a chain of bladders, which being blown up by certain liquors shorten the said springs, and so contract the muscle.

But Mr. HOOKE affirmed, that he could not discover any such texture in the said fleshy part, but that his observation was, that the fleshy part of a muscle consists of an infinite number of

[1] Mr. HOOKE's *Description of Helioscopes and some other Instruments made by him*, was printed at London, 1675, in 4to.

exceedingly small round pipes, extended between the two tendons of the muscle, and seem to end in these: which tendons in the muscles of beef boiled would be easily stripped off from the ends of those pipes, and so leave the ends of the round pipes very distinct. He said, that the reason of the moving of a muscle might be from the filling or emptying of those pipes, whose sides seem to be flexible like those of a gut.

Flight of Birds and of Man.

Feb. 11. Dr. CROONE read his discourse concerning the manner, how flying is performed by birds; showing, in order thereunto, the structure of a duck's wing and body, especially of the muscles and their insertions into the humerus.

Mr. HOOKE intimated, that there was a way, which he knew, to produce strength, so as to give to one man the strength of ten or twenty men or more, and to contrive muscles for him of an equivalent strength to those in birds. He hinted likewise, that a contrivance might be made of something more proper for the feet of man to tread the air, than for his arms to beat the air.

Feb. 15.

(23) *Animadversions on the first part of the Machina Coelestis of the honourable, learned, and deservedly famous astronomer,* Johannes Hevelius, *consul at Dantzick: Together with an Explication of some Instruments made by R. H.* 78 pp.

Cutlerian Lectures, No. 2, 1674.
Notice in *Phil. Trans.*, No. 109, p. 215, December 1674.

EXPLANATION OF PLATES.

FIG. 1. Iron Quadrant for use with telescopic sights; with Screw-frame *hhh*, made by Tompion, a watch maker in Water Lane, near Fleet St.
11. Enlarged view of Screw-frame.
12. Application of telescopic sight with cross threads to instrument.
14. Diagram to explain method of adjustment of instrument.
15. Quadrant fitted with a Driving-clock with circular pendulum to follow the stars.
20, 21. Cog-gearing between wheel and pinion.
22. Figure showing the application of a Driving-clock.
24–27. Bubble levels filled with water and nitric acid.
29. Glass plate ruled with diagonal lines for subdividing degree spaces on the rim of a quadrant.

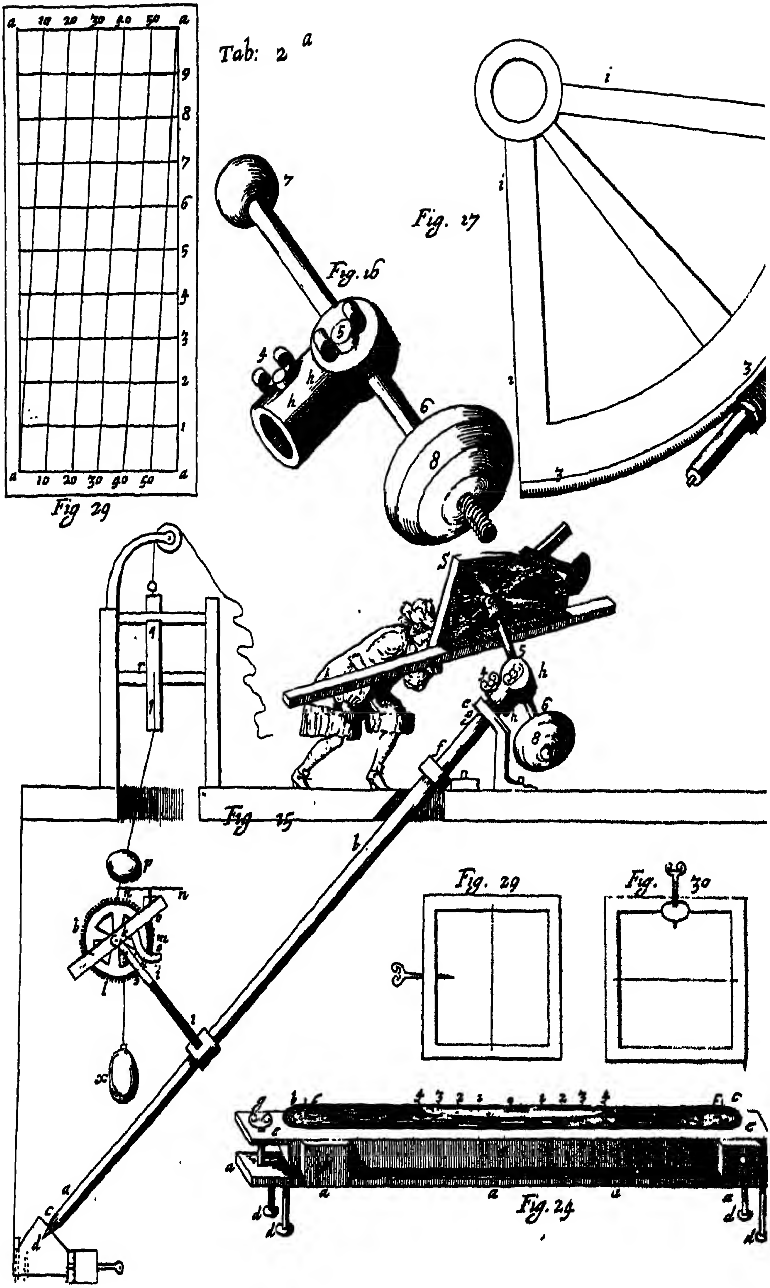

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Fig 29
Fig. 16
Fig. 17
Fig 15
Fig. 29
Fig. 30
Fig. 24

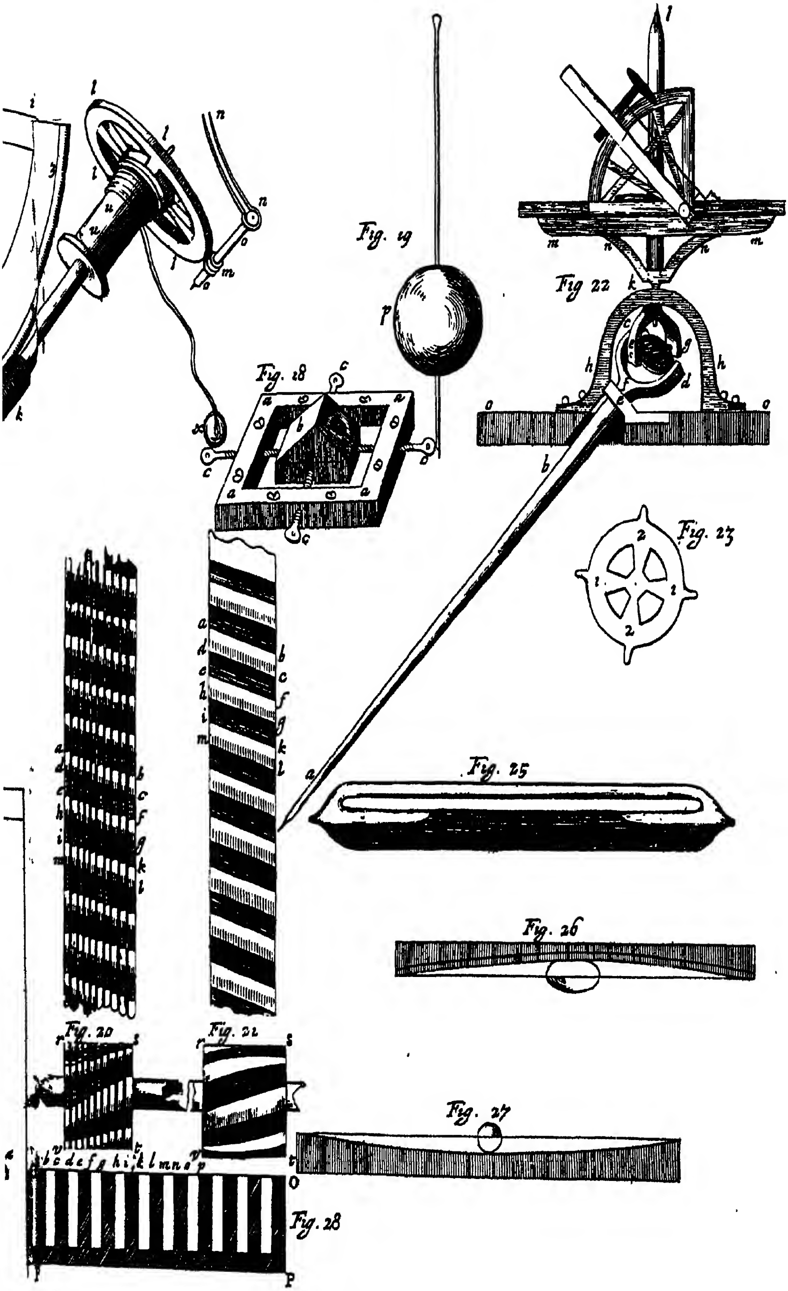

Fig. 18
Fig. 19
Fig. 20
Fig. 21
Fig 22
Fig. 23
Fig. 25
Fig. 26
Fig. 27
Fig. 28

the light and shadows upon those spots, which Dr. VOSSIUS esteemed mountains, but we believe cavities, the middlemost part of the spot being the most prominent is not first enlightened, as it ought to be, according to Dr. VOSSIUS's supposition; but the tops and sides of those circular ridges, that encompass the spot, and are next the sun, are so; and the shadow is, as it ought to be, cast regularly upon the other parts of the moon, according to the true rules of shadow: insomuch that at the beginning the whole middle of the cavity is perfectly dark, as being overshadowed by the ridge of the sunny side: but as the sun rises higher, and enlightens the bottom of the cavity, one may in several of them discover, not only divers other lesser cavities or spots, encompassed with ridges, as the greater, but also several small hills or hillocks, such as Dr. VOSSIUS would suppose inverted by the refraction of the lunar atmosphere, which appear in their true shapes, and the light and shadow properly posited.

As to the appearance of Tenerife, Mr. HOOKE conceived, that it was no other than that the shadow of the Pic darkened the surface of the sea towards the west, and likewise that part of the vaporous air, that was above the sea, as it may be very often observed, when the air is hazy, the radiations between the clouds and the shadows of the clouds are plainly distinguished in the body of the air. And that this is so seems very probable from the ensuing circumstance in that history; for, upon the rising of the sun a little higher, the said atmosphere thickened into clouds, that covered the surface of the sea and islands adjacent.

Manuring by Sea Sand.

Mr. OLDENBURG produced a paper sent in by Dr. DANIEL COXE, concerning the improvement of Cornwall by sea sand.

This discourse giving occasion to consider of the cause, that might render this sea sand more fertilizing than other sand, Mr. HOOKE intimated, that the sand being made of the sea-water, which in process of time was condensed, it seemed, that this sand not being yet quite fixed, might, by being exposed to the air, and mixed with the rain-water, be resolved into sea-water, and so fertilize the ground.

Phosphorescence and Vibratory Nature of Light.

Mar. 11. This [discourse by Mr. BOYLE about shining flesh] gave occasion to some hints for a general hypothesis for explaining the nature of light, concerning which Mr. HOOKE gave his thoughts as follows:

That light is a vibrating or tremulous motion in the medium, (which is thence called pellucid) produced from a like motion in

the luminous body, after the same manner as sound was then generally explained by a tremulous motion of the medium conveying sound, produced therein by a tremulous motion of the sounding body: and that, as there are produced in sounds several harmonies by proportionate vibrations, so there are produced in light several curious and pleasant colours, by the proportionate and harmonious motions of vibrations intermingled; and as those of the one are sensated by the ear, so those of the other are by the eye.

Mr. HOOKE intimating, that he had formerly brought in a paper concerning light, but not left it to be registered, he was desired to read it again at the next meeting.

Newton's Experiments.

He was also desired to have ready for the next meeting, the apparatus necessary for the making Mr. NEWTON's experiments formerly alleged by him, for evincing the truth of his new theory of light and colours, especially since Mr. FRANCIS LINUS had written another letter from Liège to Mr. OLDENBURG, dated February 25, 1674/5, N. S., containing assertions directly opposite to those of Mr. NEWTON.

Discourse on Light and Colour.

Mar. 18. Mr. HOOKE read a discourse of his concerning the nature and properties of light; in which were contained several new properties of light, not observed, that he knew of, by optical writers: and those were:

1. That there is an inflexion of light differing both from refraction and reflection, and seeming to depend upon the unequal density of the constituent parts of the ray, whereby the light is dispersed from the place of condensation, and rarefied, or gradually diverged into a quadrant.

2. That this deflexion is made towards the superficies of the opacous body perpendicularly.

3. That in this deflexion of the rays, those parts of diverged radiation, that are deflected by the greatest angle from the straight or direct radiations, are faintest: those, that are deflected by the least, are the strongest.

4. That rays cutting each other, in one common foramen, do not make the angles *ad verticem* equal.

5. That colours may be made without refraction.

6. That the true bigness of the sun's diameter cannot be taken with common sights.

7. That the same rays of light falling upon the same point of the object will turn into all sorts of colours, only by the various inclination of the object.

8. That colours begin to appear, when two pulses of light are blended so very well, and near together, that the sense takes them for one.

1675

Taste.

Mar. 25. Mr. HOOKE remarked, that all bodies dissolvable by the saliva are tastable, and consequently all bodies tasteless, that cannot be dissolved by the saliva.

He said further, that any body, that is saporous, hath something peculiar in its structure, which gives it a peculiar taste; and that there is probably as great a variation in taste as there is in colours.

Vossius's Reply.

Apr. 15. Mr. OLDENBURG read a paper in Latin, of Dr. VOSSIUS, containing some considerations upon Mr. HOOKE's animadversions on his former papers, concerning the spots of the moon and the Archimedean burning glasses.

Mr. HOOKE remarked, that a good observer would see, that the tops and sides of the circular ridges, that surround the spots in the moon, are enlightened *gradatim*, and not alike, and all together; which latter must be the case, if Dr. VOSSIUS's hypothesis were true.

He appealed to the members present, whether parabolical speculums were not better for burning than such flat ones, as Dr. VOSSIUS insisted upon.

Reflecting Quadrant.

Apr. 22. Mr. HOOKE was put in mind to make trials with the quadrant, for which the society had been at near fifteen pounds expense.

Helioscope.

May 20. Mr. HOOKE produced his telescope, which being directed to the sun, rendered, by divers reflections, the beams of the sun so weak, that one might look upon the sun with as little inconvenience to the eye, as upon the moon.

May 27. Mr. HOOKE promised to bring in his helioscope perfected; and likewise another experiment.

Salt Springs of Cheshire and an Underground River.

June 3. There was read out of the Register a discourse of Dr. JACKSON, a physician in Cheshire, giving an account of the salt springs of that country.

Mr. HOOKE took occasion from the mention made in that discourse of an extraordinary hole, to relate, that he had been informed by a friend of his, living at Bristol, that near that city there was a hole of an extraordinary depth, in which at a great distance from the surface of the earth there ran a river, which being sounded was found of a vast depth. He said, that he was promised a more particular account of the observables of that place.

Mr. HOOKE related, that there was a place in Cheshire belonging to the Lord BRERETON, where men having dug to a great depth for water to make salt with, but having met with none, lighted at last upon a stiff clay ground, which when they had bored into about five or six feet, the salt water from underneath the said bed of clay gushed out with such great violence, and in such a great quantity, as to fill the well, which was some hundreds of feet deep, to the top.

This gave occasion to speak of the origin of springs and rivers; several of the members being of opinion, that they were caused by rain and snow.

Motion of the Earth.

June 8. Apropos of HOOKE's Treatise on the Motion of the Earth, J. GREGORY of St. Andrews wrote to OLDENBURG an account of his thoughts on proving the Motion of the Earth.

[Rigaud, *Correspondence*, p. 262.]

Rainfall and Weather-clock.

June 10. Mr. HOOKE remarked that the [measurement of rainfall] had been proposed to the society many years before. Sir CHRISTOPHER WREN, who, by the contrivance of a rain bucket, had taken an account of the water that fell for a long time together, and by his weather-clock had, among other particulars, not only taken in the measuring the quantity of rain that fell, but also the time when it fell, and how much at each time: which gave occasion to mention the important uses to be made of that instrument, if put into practice; since it was, by some additions made thereunto by Mr. HOOKE, adapted to record the weight of the air; the drought and moistures, the heat and cold, of the weather; the quarters and strength of the winds; the rain, sunshine, &c. and all this to be performed by one motion, driving all the parts of the instrument; which was the more considerable, for that itself records its own effects.

Warping of Wood and Valves in Vegetables.

June 17. Sir WILLIAM PETTY took occasion from Dr. GREW's discourse to propose it to consideration, what might be deduced from the discourse read for explaining the cause of the warping of wood; concerning which Mr. HOOKE said, that there was

a fermentation in the liquors of wood, which required a considerable time to do its work, which was by making the liquors work upon one another, to separate the moisture, without which there was no fermentation; and which being driven out, the wood was then seasoned, and so warped not; so that by destroying the fermentative principle, the wood was preserved, and made to retain its figure, and so kept from warping.

Mr. HOOKE added, that it was worth a more particular inquiry, whether there were not valves, or something analogous to them, in vegetables.

Helioscope and Sunspots.

June 24. Mr. HOOKE produced again his helioscope, which had three reflections; the first reflecting the fifth, the second the twenty-fifth, and the third the hundred and twenty-fifth part of the direct light of the sun to the eye.

It being asked, whether by it he had observed any spots in the sun, he answered, that at present there were none, that he could see.

Gresham College.

June 28. Inquiry being made, whether the west gallery in Gresham College was emptied, it being found not to be so yet; Mr. HOOKE was ordered to call upon the officers of the East India Company to remove their goods, according to the order of the committee of the said company.

Resistance of Air.

July 8. Mr. HOOKE showed an experiment concerning the resistance of air to a ball moved with and without an expanded area; of which he was desired to bring in a particular account in writing.

Aug. 24. LETTER FROM HOOKE TO JOHN AUBREY.

These for my much hon[d] freind Mr. Aubrey at Chalke.

To be left at the Lamb in Katherine street in Salisbury.

Dear Dr. Aubery, I received your extraordinary kind letter and acknowledg myself very much obleiged to you for your kind advertisement concerning the young man, brother to Mr. Snell. I should be very glad to have him live with me, if he be a sober virtuous young man and diligent in following such things as I shall imploy him about, which I doubt not will be much for his good hereafter. If he be with me I would have him upon these conditions. 1[st] That he will ingage to stay with

me for a certaine time. Next that his friends will provide him with clothes, or allow so much towards it as shall be equivalent. And my reasons are because though he do me service yet I shall assist him much more, and therefore I think it will be enough for me if I take him with nothing if I find him meat, drink, lodging washing & instruction. And my reason for obleiging himself to stay with me is that I can have enough that will doe it, and it will not be reasonable that after I have been at the paines to fit him for the doing my business he should presently leave me to seek a new one to be taught. My entertainment will be but plain but he shall not want any thing necessary for his diet, lodging or studys. Let me hear from you speedily because I am just agreeing with another, but I like the ingenuity of this boy, if all other conditions please. I would not have him for less than 7 years. I have preferred Harry to Mr. Mountaine who will imploy him these two or 3 years, and is so pleased with him that he hath promised to bear his charges to Italy that he may see and improve himself. And I doubt not but he will with ease get his £150 or £200 per annum. He hath lately done some peices extraordinary well, & will be paid for them accordingly. I have been severall times with Mr. With but have not spoken with him about ye R. S. but I will if I have an opportunity. Twill doe very well surely if you write to him; You best know how. I dream (?) much at setting up a select clubb, whether 'twill take I know not. As we are, we are too much enslaved to a forreine spye, and think of nothing but that, and while 'tis soe I will not doe any [thing] towards it. I have many things which I watch for an opportunity of publishing, but not by the R. S. Oldenb.[urg] his snares, I will avoid if I can

Dear S[r]. I am your most affectionate, humble servt.

August the 24 1675. R. H.

Pray doe me the favour to let me hear of you by next post.

College of Physicians.

Oct. 13. Hooke received a final fee of £20 (£120 in all) for his work on the building of the College of Physicians in Warwick Lane. (*Cash book, College of Physicians.*)

Lamp.

Nov. 4. Mr. Hooke read a lecture, wherein he explained a mechanical contrivance to supply the pabulum of a lamp in

the same degree it is consumed, or to keep the surface of any liquor, fit to feed the flame of a lamp, always at the same height, till all be wasted. Having both described and delivered one way of performing this, he promised to bring in divers other ways of effecting the same thing.

Nov. 11. Mr. HOOKE read another lecture about divers ways of keeping the pabulum of a lamp always at the same height with the bottom of the flame thereof, till all the liquor be consumed. Of these methods he explained seven or eight more.

Nov. 24.

(25) *A Description of Helioscopes, and some other Instruments made by R. H. With a Postscript.* 32 pp.

Cutlerian Lectures, No. 3, 1675. Oldenburg's review and reply in *Phil. Trans.*, No. 118, p. 440, October 1675.

EXPLANATION OF PLATES.

1. A Helioscope which shall so take off the brightness of the Sun, as that the weakest eye may look upon it, at any time, without the least offence.

This was achieved by multiple reflection from the surfaces of black Glasses.

FIG. 1. A 60-foot object glass contracted into a 12-ft. tube by the help of 4 several reflecting plates according to a method shewn to the R. S. in 1668.

FIGS. 2, 3. Telescopes with $\frac{2}{3}$ taken off the length of the tube.

FIG. 4. Scheme for a horizontal telescope.

FIG. 5. Instrument to reduce or destroy colouring and haziness of images by the use of glass reflecting plates *cg* and *dθ* whose surfaces are not parallel.

FIG. 6. Alternative scheme for a horizontal telescope.

FIG. 7. Reflecting telescope.

FIG. 8. Letters same as those to 1st and 11th figs. of *Animadversions*.

FIGS. 9 and 10. The five parts of the *Universal Joynt*.

FIG. 11. Instrument for describing all manner of Dials by the *Tangent projection*, with 2 axes connected by a Universal Joint. *dd*. Axis 11 to Axis of Earth.

FIGS. 12, 13. Manner of contriving centres and sockets for telescope screws.

FIG. 14. A telescope tube of $\frac{1}{3}$ of focal length of object glass, with a diagonal mirror near eyepiece.

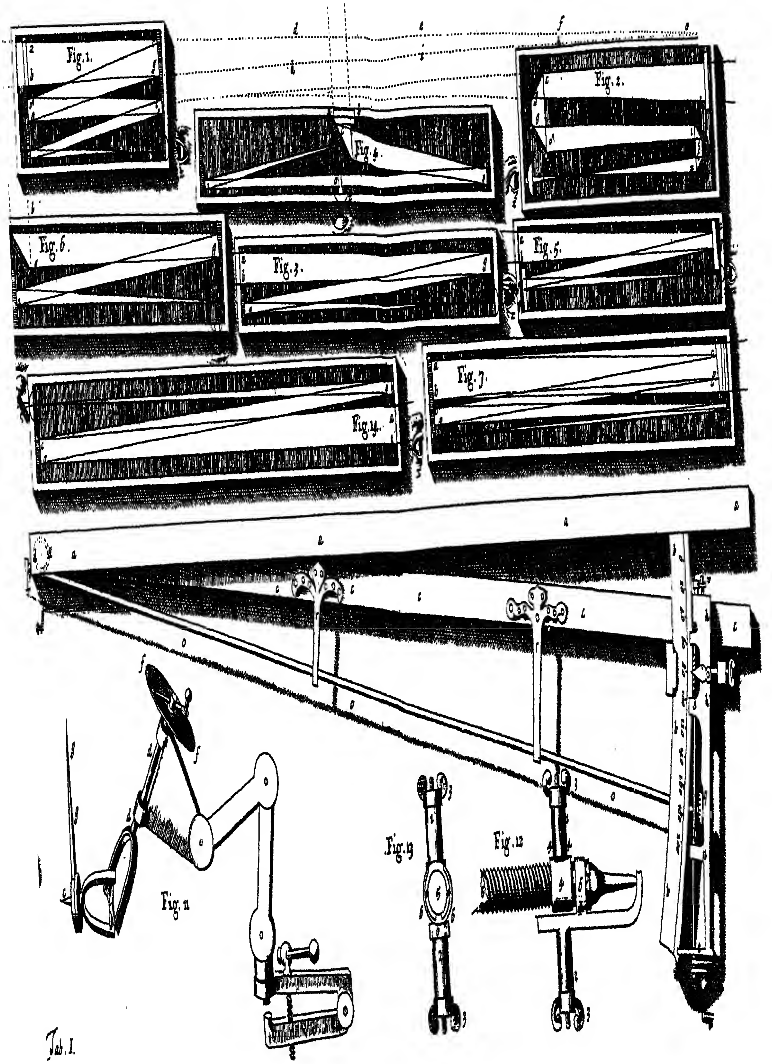
Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
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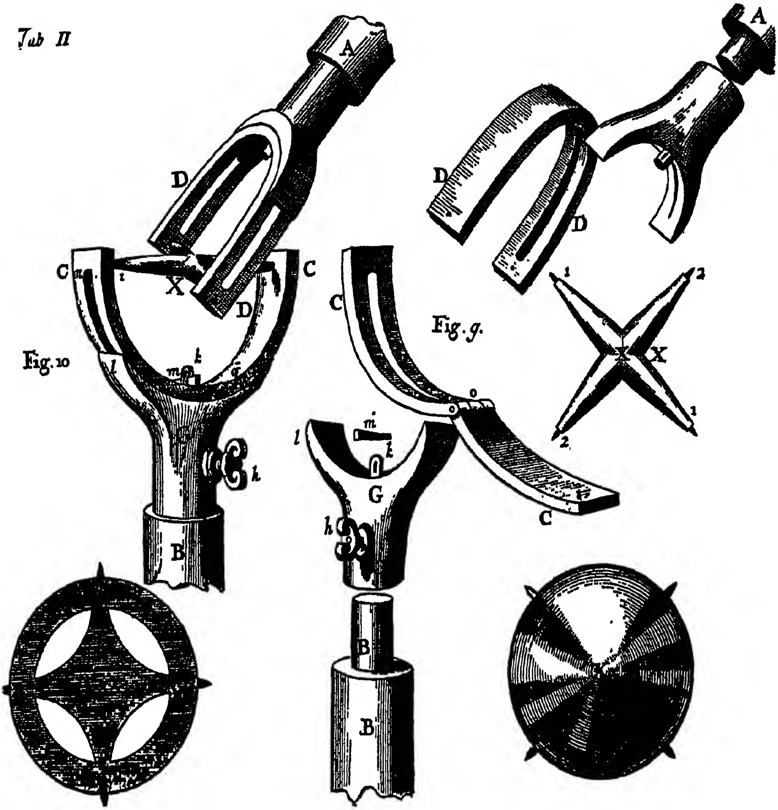
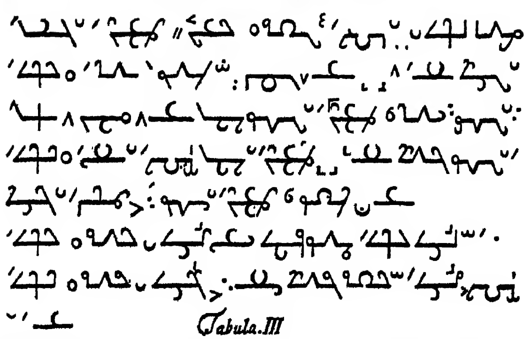
Fig. 11
Fig. 12
Fig. 13
Tab. I.



HOOKE'S UNIVERSAL JOINT.



EXAMPLE OF THE UNIVERSAL AND REAL CHARACTER INVENTED BY DR. WILKINS.

'A character and language perfectly free from all manner of ambiguity, the most easie to be understood and learnt in the world.'

Loadstone.

Mar. 2. Mr. HOOKE mentioned, that there had been lately with him a person, who had suggested to him some new notions concerning the loadstone, viz. that the motions of it would vary and change far otherwise than hitherto had been taken notice of, so as that the variation of it would be for a quarter of a year to the east, and the next quarter to the west of the north; and particularly that the twenty-ninth of February preceding it would be four degrees to the eastward. Mr. HOOKE added, that he intended to observe it, having already made a meridian for that purpose, and to give the society an account of the result of his observations.

1676

Bedlam.

It will be noticed that the notices of HOOKE's scientific work during 1675–6 are unusually scanty. This was partly due to his dispute with OLDENBURG, but also to his being occupied with the building of the great Bethlem Hospital, which was begun in 1675 and completed in 1676. He was engaged upon Montague House at about the same time.

The only architectural drawing by HOOKE that is known to me is the rectangular ground-plan of an unnamed house. There are two staircases and four rooms on the ground floor, with seven windows both front and back. [MS. *Sloane*, 1039, f. 167.]

Prism Experiment.

Apr. 6. A committee, consisting of Sir JONAS MOORE, Dr. CROONE, Mr. HILL, Dr. GREW, and Mr. HOOKE, was appointed to try Mr. NEWTON's experiment controverted by Mr. LINUS; and it was ordered, that after the trial of it by that committee, it should be made before the society.

Apr. 27. The experiment of Mr. NEWTON, which had been contested by Mr. LINUS and his fellows at Liège, was tried before the society, according to Mr. NEWTON's directions, and succeeded, as he all along had asserted it would do.

May 11. EVELYN noted in his *Diary* that he 'dined with Mr. CHARLETON and went to see Mr. MONTAGUE's new palace near Bloomsbury, built by Mr. HOOKE, of our Society, after the French manner'. He saw it again on 5 Nov. 1679.[1] This house became the British Museum.

[1] On 10 Oct. 1683 he described the house as 'stately and ample' with fresco paintings by VERRIO which exceeded anything he had yet done. 'The garden in large and in good air, but the fronts of the house not answerable to the inside. The court at entry and wings seem too near the street.' (Evelyn, *Diary*.)

ENTRANCE TO MONTAGUE HOUSE

From a sketch by N. Rooker, 1778, in the British Museum

June 10.

'Mr. Hook hath presented us with a mechanical contrivance shewing how to supply ye pabulum of a lamp in ye same degree it is consumed, that is to say to keep ye superficies of any liquor fit to feed ye flare of a lamp alway at ye same height, till all be wasted.' *Letter* from Oldenburg to Lister. June 10, 1676.

Dispute Between Hooke and Oldenburg.

Oct. 3. Oldenburg's Advertisement printed on page 47 of the *Philosophical Transactions.*

Oct. 12. At a meeting of the Council of the Royal Society it was ordered that the Society's printer, Mr. Martyn, be required to give notice in the *Philosophical Transactions* next to be printed, of what the council was informed he had declared, viz. that the tract called *Lampas,* made by R. Hooke, F.R.S. and lately printed by John Martyn printer to the said society to which is annexed a postcript reflecting on the publisher of the *Transactions,* was printed without the leave or knowledge of the council of the R. S., and that the said printer had seen nothing of the postscript thereof before it was printed off, nor knew any ground for the aspersions contained therein: and in case the said printer should refuse to obey this order, that then the new printer, whom the president hath power to constitute in his room, be required to signify to the public in print, that Mr. Martyn was removed for disobeying this order of the council. (See p. 47.)

Sanative Spring in Staffordshire.

Nov. 2. Mr. Hooke mentioned a new sanative spring lately discovered in Staffordshire, healing divers diseases, as the dropsy, scurvy, &c. adding, that a book was printed concerning it.

Dipping Needle.

Dec. 7. Mr. Hooke showed some magnetical experiments.

It was ordered, that these experiments be prosecuted; and that the committee formerly appointed to take care of the repository be desired to have a good inclinatory needle made, and suspended in the society's repository, to see what change there would be in it in tract of time; as also to observe, whether this dipping-needle answers the latitude from the pole of the world.

Dec. 14. Mr. Hooke, upon account of the presence of Sir Joseph Williamson, one of the principal Secretaries of State, repeated his suggestions, made at the last meeting, concerning the dipping-needle, viz. that it was very difficult to find what the inclination of the needle should be; and there being no certain way of knowing the needle's inclination, there could be no certainty of knowing the longitude thereby.

He said, that, according as the needles are longer or shorter, they have different inclinations; and that it is not known what distance the dipping-needle must have from the magnet for such or such a latitude.

He added, that if we have but the true variations of two places, whose longitudes are exactly known, that will give us the magnetical pole: and then if the variation be true, and the pole given, we need no dipping-needle, because we shall then be able to tell where the needle must dip.

Publication Committee.

Dec. 21. A motion being made, that the Register and Letter Books of the society might be reviewed, in order to see what might be fit to be published; it was ordered, that Dr. WHISTLER, Dr. CROONE, Mr. HILL, Mr. OLDENBURG, and Mr. HOOKE, or any three of them, be a committee for that purpose; and that they acquaint the President with the particulars, which they shall have thus selected.

1676/7

Astronomical Instruments to be lent to Greenwich.

Jan. 25. It was ordered, that the astronomical instruments belonging to the society, and being in their repository at Gresham College, be lent to the observatory at Greenwich, for making astronomical observations; and that Mr. HOOKE's new quadrant be forthwith finished at the charges of the society.

Dipping Needle.

It was moved, that the making of the inclinatory needle formerly spoken of, should be hastened by Mr. HOOKE, in the exactest manner, that could be.

Mar. 1. Another inclinatory needle was tried, and proved to be better than the former. However, the artist was still pressed to try to make another yet more exact.

1677

Catalogue of Arundel Library.

Sept. 13. It was ordered, that Mr. HOOKE agree with Mr. FORSTER for making the catalogue of the Arundel Library: and that what he shall agree for, be paid by the Treasurer.

Density of Liquids.

Oct. 15. The President not coming, the Vice-President Mr. HENSHAW and the rest of the society desired, that an ex-

periment might be shown; which was accordingly done by Mr. HOOKE; the effect of which was to show a very easy but exceedingly curious way to examine the comparative weight of liquors, and that to so great a niceness, as very sensibly and manifestly to exhibit such weight of two liquors, though they differed from one another but a thousandth part of their weight. This was performed by the help of a large glass of a pear-like form, equalling in bulk about three pounds of water, which by that included in it was made almost equiponderant to the water, but yet somewhat heavier, that it might sink to the bottom; but by the finest hair tied to the stalk could be suspended in the water. This hair was tied to the scale of a beam, and this poise by a counterpoise in the other scale was made to swim in the water so, as neither to touch the bottom nor the top; and when so poised, it was found, that a fifth part of a grain added to or taken from the scale would make the glass pear rise to the top or sink to the bottom. Whence it was evident, that the whole glass weighing about four pounds, which amounts to 22,040 grains, or 220,400 tenth parts of a grain, one single tenth part of a grain would turn it. And the glass, when suspended, being always equal to equal bulk of water, if that might be altered, the poise must be altered; and consequently by help of the scales be made sensible. This experiment and the nicety thereof being understood by the members present, it was desired, that trials might be made at the next meeting upon several sorts of water, as pump-water, New River water, Thames water, and rain-water, that so they might be experimentally satisfied of the exactness of this new instrument; which was new upon this account, that it had not been taken notice of by any of those, who had written of this subject, such as, GHETALDUS, SEVINUS, PASCAL, &c. they having only taken the comparative weight of some small counterpoise within and out of the same liquor, which they had always performed with the same scales; which are no ways fit for exhibiting the niceness and curiosity of this experiment. Several objections were made and answered, and Dr. WHISTLER explained some things in it, which seemed a little difficult. [An alternative account is given on p. 445.]

Hooke Invited to take his Place at the Table.

This debate being over, it having been concluded in some foregoing * * the Vice-President Mr. HENSHAW with the rest of the members desired Mr. HOOKE, Mr. OLDENBURG having died since the last recess, to take his place at the table, and to take an account of such considerable matters, as should be shown or discoursed of at the meetings of the society; which he accordingly did.

New Leather.

Mr. HOOKE acquainted the society with a new sort of leather found out at Paris, and made impervious to air and water; so that therewith had been made all sorts of riding and wearing apparel to keep out wet, cups and borachios to hold or carry liquors, beds to lie on, and floats to swim with, which hold air like a bladder, covers of tents, coaches, sedans, mails, &c. floors to tread on dry in marshy places, boots to wade in, &c. And he presented from Mr. BOYLE a cup made of the same leather, which, he affirmed, held water six days without soaking through the leather, though as thin as a Corduban glove; and also supple.

Mr. PACKER remarked, that it was fit for hat-cases, stockings, &c.

Mr. HOOKE mentioned, that he conceived it to be done only by soaking the leather in a mixture made with salad oil, and well boiled together. He was desired to make trial of this way against the next meeting.

Viviparous Onion.

Mr. HOOKE showed a sort of Portugal onion, which he had received from Dr. WHISTLER, who called it viviparous; the said root sending up a stalk, upon the top of which grew, instead of seed, a cluster of very small onions exactly like the root; each of which put into the ground would increase and produce such another bunch of small onions. An account was ordered to be taken of it, none of the like having been seen before by any of the members present; and the onion was returned.

Leewenhoeck's Correspondence and Microscope.

Mr. HOOKE read a letter which had been delivered him by Mr. HENSHAW to whom it was sent by the President, in which Mr. LEEWENHOECK understanding, that Mr. OLDENBURG was dead, desired to know, to whom he might address his letters for the future; enclosing several testimonials of his former experiments, and an account both in Dutch and Latin of some new observations.

The consideration of this was adjourned to the next meeting. And in the meantime Mr. HOOKE was desired to make a microscope after a way, which he proposed as very likely to do as much, if not in the same manner as that of Mr. LEEWENHOECK.

Eclipses of Saturn.

Mr. HOOKE produced an ephemerides of twelve eclipses of Saturn by the moon, together with the transit of through the sun, and a calculation of eclipses for the two succeeding

years; one of which was omitted in HECKER, which was enclosed in a letter to him from of Hamburg; some copies of which he promised to deliver to some astronomical observers.

Cider-making.

Mr. HOOKE related, that had affirmed, that by rasping his apples with a bread-grater he was able to make almost a third part more of cider than by the common way. He mentioned likewise an expedient of rasping those apples much easier by help of a cylinder covered with tin plates made of the form of a grater. Mr. PACKER objected, that though by that means a third part more of liquor was obtained, yet the quantity of lees after settling would be so great, that little more clear cider would be made that way than by the common.

Oct. 25.[1] DERHAM'S *Alternative account of the Experiment of the Density of Liquids.*

It was a very easy way to examine the comparative weight of liquors, and that to so great a niceness, as very sensibly and manifestly to exhibit such weight of two liquors, though they differ'd from one another, but a 100000th part of their weight.

This was performed by the help of a large glass, of a pear-like form, equalling in bulk about three pound of water; which, by shot included in it, was made almost equiponderant to water; but yet somewhat heavier, that it might just sink to the bottom; but by the finest hair, tied to the stalk, could be suspended in the water. This hair was tied to the scale of a beam; and this poise, by a counterpoise in the other scale, was made to swim in the water, so as neither to touch the bottom nor the top. And when so poised, it was found, that a 5th part of a grain added to, or taken from the scale, would make the glass-pear rise to the top, or sink to the bottom. Whence it was evident, that the whole glass, weighing about four pounds (which amounts to 22040 grains, or 220400 tenth parts of grains) and that one single tenth part of a grain would turn it. And the glass, when suspended, being always equal to an equal bulk of water, if that weight be alter'd a 220400th part, the poise must be alter'd, and consequently, by help of the scales, be made sensible.

This experiment, and the nicety thereof being understood by

[1] Probably in error for Oct. 15. See p. 442.

the company, it was desir'd, that trials might be made the next day upon several sorts of water, as pump-water, new river water, *Thames* water, and rain-water, that so they might be experimentally satisfied of the exactness of this new instrument: which is new upon this account, that it hath not been taken notice of by any of those who have written on this subject; as *Ghetaldus*, *Stivinus*, *Paschal*, &c. they having only taken the comparative weight of some small counterpoise within, and out of the same liquor, which they have always perform'd with the same scales, which are no ways fit for exhibiting the niceness and curiosity of this experiment.

On *November* the first following, the experiment was accordingly made, and it was found, that two grains of salt, being put into two gallons of water, caus'd the counterpoise to be considerably lighter: which was found to be so, upon repeated trials. [Derham, p. 126.]

Organisms Viewed in Capillary Glass Tubes.

Nov. 1. There were produced a great many exceedingly small and thin pipes of glass of various sizes, some ten times as big as the hair of a man's head; others ten times less. These were made, in order to try a conjecture of Mr. HOOKE propounded to the society, that the discoveries, affirmed to be made by Mr. LEEWENHOECK, were made by help of viewing with a good microscope such small pipes containing the liquor or water, in which those multitudes of exceedingly small insects or animals wriggling among each other are discovered; for that he alleged, that the said pipes being filled with liquors became themselves as it were magnifying glasses, augmenting such bodies, as swim in the said liquor, on those parts of the said pipes, which are farthest from the eye-glass; for the pipes themselves being looked on by the help of a very good microscope, are made very large and conspicuous; and they again augmenting the opposite parts by the refraction on their cylindrical surfaces double the effect of a single microscope, as was very evident. But notwithstanding this there was no discovery made in the liquor, that was made use of, which was only common pump-water, of any such minute animals. It was therefore ordered, that against the next meeting pepper-water should be provided, and some better microscope than that made use of, that the truth of Mr. LEEWENHOECK's assertions might, if possible, be experimentally examined, of which he had produced so many testimonies from such, as affirmed themselves to be eyewitnesses.

Prepared Leather.[1]

Mr. HOOKE produced a piece of leather, which he had made in imitation of the impenetrable leather of the French. This was more supple than the French leather; and it was judged, that the French had more wax in its composition than this now produced; and by the smell the French seemed to be made of some ingredients of a more pleasing smell than this of Mr. HOOKE, which smelled more strongly of the boiled salad oil and beeswax boiled together for an hour or two. This leather, though very limber, was found to hold water for some time without being wet through: but whether it would hold so well as the French, further trial was to be made; as also of some other composition for soaking the skins in, that they might smell more like those from France.

Density of Liquids.

After this the third experiment was shown by Mr. HOOKE to verify the truth of his former assertions concerning the exceedingly great curiosity of the new contrived poise for examination of the comparative weight of liquors even to the hundred thousandth part of their bulk: and it was before the whole society evidently shown, that the eighth part of a grain would manifestly turn that scale, and make it preponderate, in which it was put; and so move a body, that was about four pounds in weight, either upward or downward. Now there being in four pounds weight 176,320 eight parts of a grain, it thence follows, $\frac{1}{176320}$ part of the weight of the water was thereby discovered; which is almost beyond imagination. And it was further asserted, that this niceness might be as much further augmented, as should be desired, or was necessary for any manner of curious trials, which was done by making the poise so much larger; and that in such trials, where great quantities of liquors would be troublesome to obtain, it was demonstrated how a receptant vessel should be contrived, that with some ounces of liquor the examination might be made of a poise of ten, twenty, or more pounds in weight, even to the accurateness of the tenth part of a grain. And to make it evident, that a small alteration of the water would be made sensible by this poise, about the quantity of two grains of salt was put into about two gallons of water; and it was apparent to all the spectators, that the poise grew very remarkably lighter. The same was also repeated; and by a second trial with the like effect the same was verified. Some further trials would have been made; but it being late, the society rose, and left the further prosecution of these experiments to some other time.

[1] Cf. R. S. MS. No. 52.

Pepper-water.

Nov. 8. The first thing exhibited was the experiment charged on Mr. HOOKE at the last meeting, of examining pepper-water with better microscopes and thinner and small pipes. The fabric of the microscope for holding such pipes was new and more convenient and expeditious for such examinations than the usual forms, consisting wholly of pieces, which slid anyways very easily, and would stand fixed and steady in any posture, and admit light to the object every way: by the comparing of which various ways of enlightening the object one might the more easily and certainly discern the true shape and constitution of any body. But notwithstanding the pepper-mixture was very strong, being made of rain-water and whole black pepper steeped in it for two or three days; and notwithstanding the microscope was much better than that shown at the last meeting; yet nothing of Mr. LEEWENHOECK's animals could be seen.

Dr. WHISTLER conjectured, that these small imagined creatures might indeed be nothing else than the small particles of the pepper swimming in the water, and no insects. But Dr. MAPLETOFT answered, that Mr. LEEWENHOECK affirmed, that he had shown them both alive and dead; dead, when he put vinegar to the pepper-water. However Mr. HOOKE upon examining the said water in the pipe with a microscope found a vast quantity of small dust of pepper moving up and down in the water.

Measuring Microscopic Objects.

He then showed a way of measuring the bigness of any object seen through the microscope, which was by opening the other eye, and seeing some other object with the left eye, whilst the right eye sees the object through the microscope: and it was evident, that a pipe not bigger than a pig's bristle appeared a cylinder of about three inches diameter: and it was suggested, that there was some hope of producing at the next meeting a microscope, that would magnify much more, and make the parts of the object more distinct.

Impervious Leather.

Mr. HOOKE produced a second trial, which he had made upon leather, for rendering it impervious to water. This was a piece of washed leather well soaked in a composition of wax and oil of turpentine boiled together. This was found very limber, and yet very close and impervious to water, the water, which was put into it, slipping from it like quicksilver on paper without sinking into or adhering to it. And it was conjectured, that this might perform much the same effect with that of the French invention. Mr. HENSHAW conceived, that spermaceti, white wax,

and pomatum being mixed with the composition above mentioned might consolidate and toughen the said mixture.

Silkworms.

Dr. Grew remarked, that silkworms' eggs had the worm within them to be seen through the shell.

Mr. Hooke affirmed, that he had seen them come out of them alive, and the rest of the shell remaining to stick to the place, where it was first laid.

Improvements in Microscopy.

Dr. Croone remarked, that the chicken might be seen formed in the cicatricula of the egg, by the help of the microscope. He was desired to show this, as soon as he could conveniently, at a meeting of the society. He complaining of the defect of microscopes for such uses, Mr. Hooke suggested some further improvement of that instrument by making use of the convexity of the surface of the liquor itself (put upon the plates of Muscovy glass) for augmenting the body within the liquor; as also for augmenting the body beyond it. The same might be done by small drops of fluids, that fall on the leaf of coleworts or any oiled or greased surface; as also by the small drops at the end of small pipes, or sticking on small threads of glass or a single clew of silk, the said globular transparent bodies being viewed by the help of good microscopes. Upon this occasion Mr. Hooke mentioned again his way, which he had formerly acquainted the society with, of making microscope glasses with small drops of glass made by melting up the ends of threads in the flame of a candle into a globular figure, and then grinding all away upon a flat except a very small segment of the spherule; and so made use of as of a plain common glass, either for a single or compound microscope. He was desired to show some specimen of this at the next meeting.

Palmetto.

Mr. Hooke promised to endeavour to procure some of the wood of the palmetto or cabbage-tree of Barbados, from Mr. Hart.

Water-poise.

Dr. Croone made an objection from some assertions of Galileo, in his book *De Insidentibus Humido*: but upon discoursing the matter it was found not to contradict anything, that was asserted by Mr. Hooke concerning the same, but appertained to some disputes about the quantity of water raised by the sinking of the poise.

Insects in Pepper-water.

Nov. 15. The first experiment there exhibited was the pepper-water, which had been made with rain-water and a small quantity of common black pepper put whole into it about nine or ten days before. In this Mr. HOOKE had all the week discovered great numbers of exceedingly small animals swimming to and fro. They appeared of the bigness of a mite through a glass, that magnified about a hundred thousand times in bulk; and consequently it was judged, that they were near a hundred thousand times less than a mite. Their shape was to appearance like a very small clear bubble of an oval or egg form; and the biggest end of this egg-like bubble moved foremost. They were observed to have all manner of motions to and fro in the water; and by all, who saw them, they were verily believed to be animals; and that there could be no fallacy in the appearance. They were seen by Mr. HENSHAW, Sir CHRISTOPHER WREN, Sir JOHN HOSKYNS, Sir JONAS MOORE, Dr. MAPLETOFT, Mr. HILL, Dr. CROONE, Dr. GREW, Mr. AUBREY, and divers others; so that there was no longer any doubt of Mr. LEEWENHOECK'S discovery. Notice was ordered to be taken of this discovery, and further trial was desired to be made upon rain-water alone; and upon rain-water, in which had been steeped, wheat, barley, and other seeds and grains: as also that blood and several other liquors should be after the same manner examined. The shape of the microscope and the manner of examining the * *.

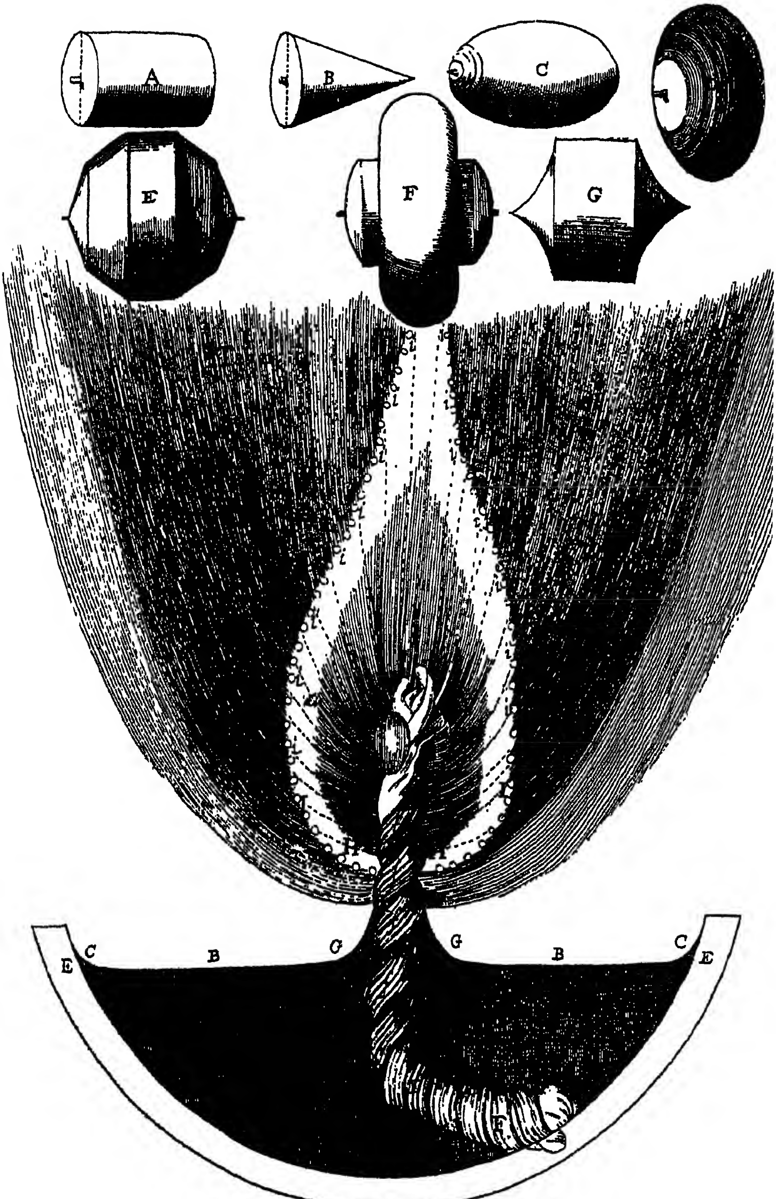
The entry of the minutes of this meeting in the Journal Book, vol. vi, p. 11, breaks off abruptly here: nor is there any entry of the minutes of any following meetings till that of December 6, 1677.

Nov. 22.

(26) *Lampas*, or, *Descriptions of some Mechanical Improvements of Lamps and Waterpoises: Together with some other Physical and Mechanical Discoveries: With a Postscript in reply to Mr. Oldenburgh.* 54 pp.

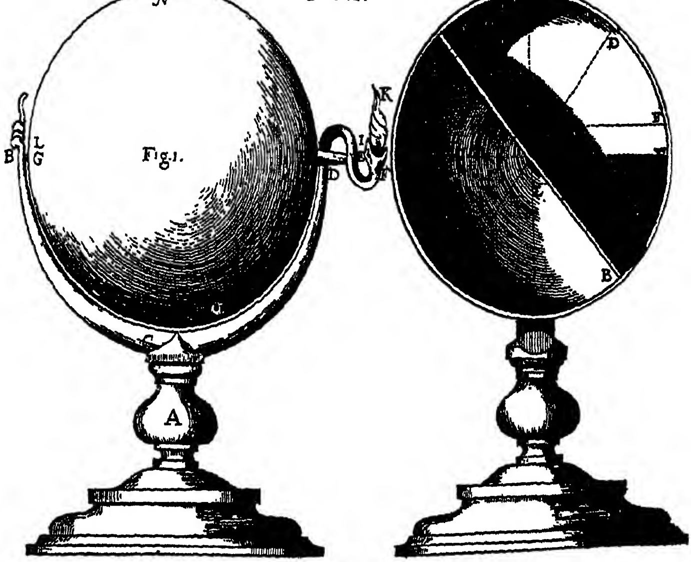
Cutlerian Lectures, No. 4, 1677.

The illustrations to this remarkable paper are reproduced on pages 451 to 455.

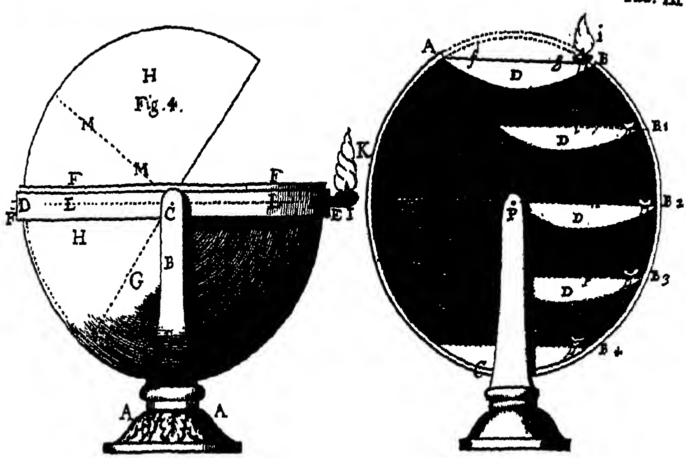


ILLUSTRATIONS TO 'LAMPAS'.
FIG. 3. Structure of Flame, Wick, and Oil in a Lamp. A–G. Alternative designs for the body of HOOKE's Improved Lamp.

Tab.1.



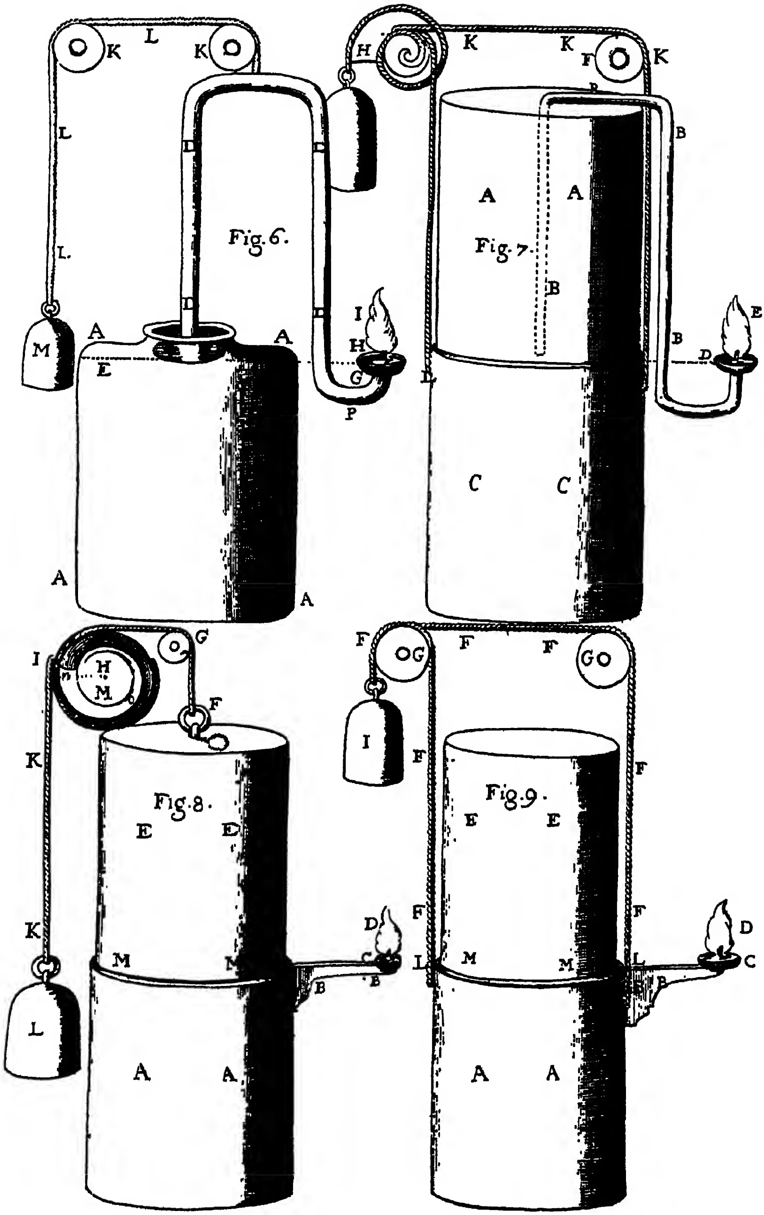
Tab. III



HOOKE'S ALTERNATIVE DESIGNS FOR AN IMPROVED LAMP.

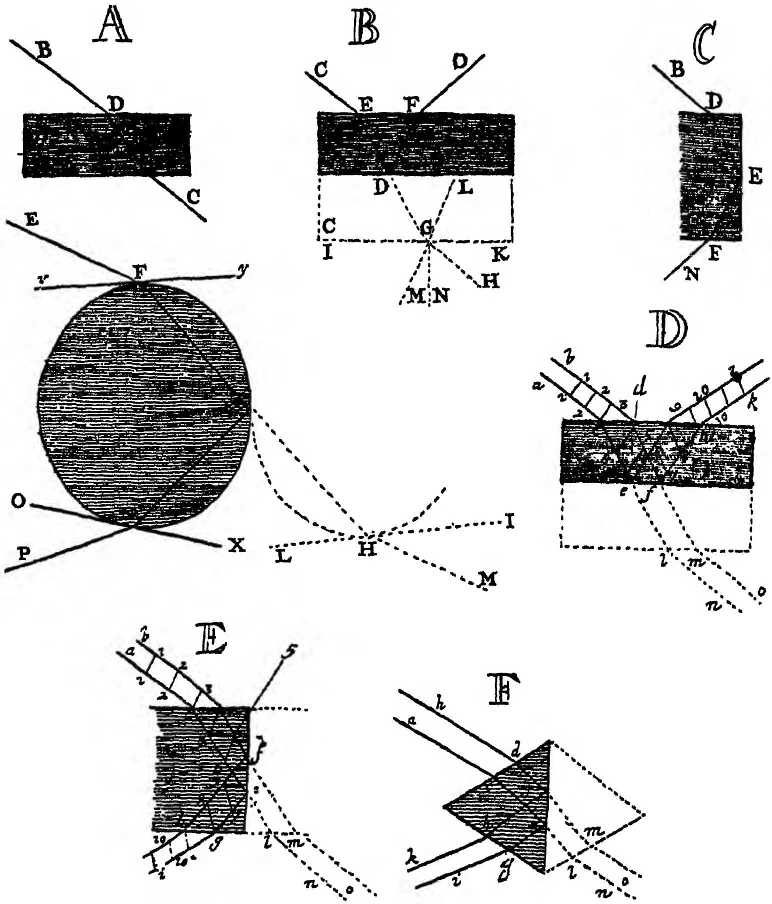
FIGS. 1, 2. HOOKE'S Counterpoised Lamp for maintaining the oil at a constant level. (1st Scheme.)

FIGS. 4, 5. HOOKE'S Improved Lamps. (2nd and 3rd Schemes.)



HOOKE'S IMPROVED LAMPS.

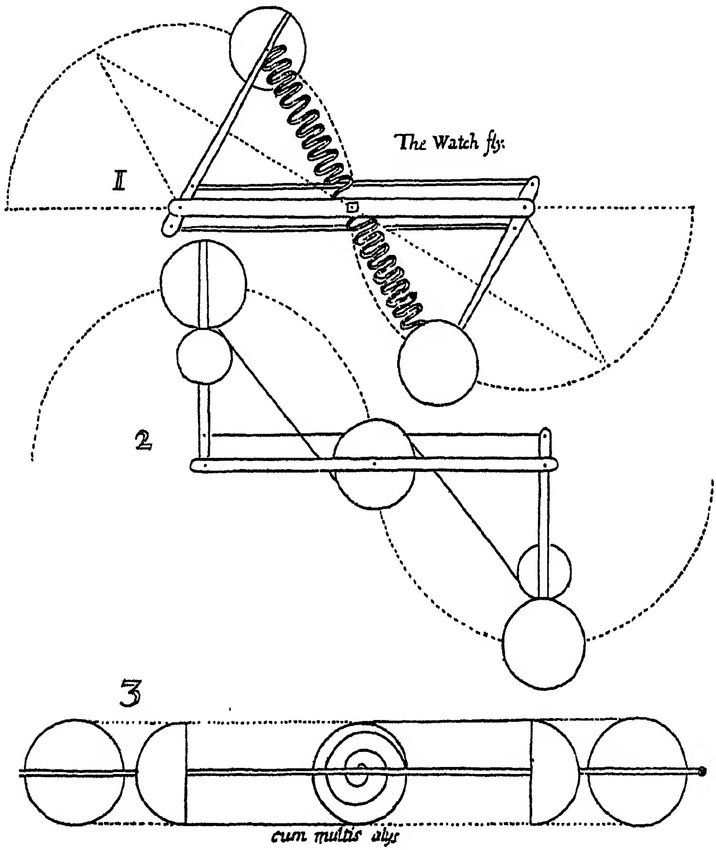
FIG. 6. 5th Scheme.
FIG. 7. 6th Scheme.
FIG. 8. 7th Scheme.
FIG. 9. 8th Scheme.



DIAGRAMS OF REFLECTION AND REFRACTION.

After his discourse on the several ways in which Lamps can be improved HOOKE proceeded to discuss certain problems of Hydrostatics and the Hypothesis of Gravitation, and finally the Laws of Reflection and Refraction of Light, and the production of Colour. His argument is illustrated by diagrams A–F.

Then follows *A description of a new sort of Clepsydra*, the outcome of the work on Lamps, and figures to illustrate *A New Principle for Watches*, shown to the Royal Society some ten or twelve years previously, and since published by HUYGENS.



BALANCE WHEELS FOR WATCHES.

The remarkable Tract then concludes with *An Observation about the Seed of Moss*, with a letter from W. C. of Bristol; an account of *Maculae in Sole*; and finally the celebrated *Postscript* on the supposed nefarious proceedings of OLDENBURG in the matter of HOOKE's discoveries.

Insects in Pepper-water.

Dec. 6. Mr. HOOKE then showed two microscopical experiments, which he had promised at the preceding meeting.

The first was a further improvement of the compound microscope, whereby he showed those small insects in pepper-water very much more magnified and more clear than they appeared the day before; which was done by making the object glass of a much smaller sphere than the last, which was viewed by several of the persons present.

Single Microscope.

The second was a new sort of single microscope, wherewith he exhibited to the President himself, and afterwards to most of the members present, the same little creatures swimming to and fro in the pepper-water contained in the small cane; and made them so visible, that all, who looked through the said microscope, though they had not been accustomed to the use of glasses, yet discovered them so plainly, as to be able to discover their figures, magnitudes, and motions. And all concluded the appearance this way to be much more clear and distinct, than it was the other way by the double microscope, though that was one of the best of that kind.

Mr. HOOKE did not now give a description of his single microscope, as having some further improvements thereof to exhibit in some of the following meetings, undertaking to make the same to magnify objects a thousand times more than this; though this, considering the clearness, magnified about a thousand times more than the common microscopes. He was desired to prosecute these improvements with what expedition he could, and to prepare a letter to Mr. LEEWENHOECK against the next meeting.

Earthquake and Storms causing Barometer to Fall.

Dr. WALLIS remarked, that in the earthquake, which happened at Oxford in the year 1665, he had observed a very considerable and sudden fall of the baroscope, though he himself did not observe the concurrent accident of the earthquake.

Mr. HOOKE affirmed, that he had for fifteen or sixteen years past constantly observed the baroscope, and that he had always found, that in the said instrument the quicksilver was always exceedingly low, and fell to that station very suddenly, whenever any considerable storm of wind and rain had happened in that time. And that whenever the quicksilver was observed to fall suddenly very low, it had always been a forerunner of a very great storm suddenly to follow, sometimes within twelve hours;

and therefore he hoped, that this instrument might be of very good use at sea, in order to the foreshowing an ensuing storm.

He also mentioned, that he had an hypothesis, by which the phenomena of the baroscope would be more clearly and distinctly made out than by any, which he had hitherto heard of; viz. of dividing and distinguishing the two principles or causes, which actuate the said instrument, from which distinction it would be easy to show the reason, why at different times the same height of the quicksilver foreshows different constitutions of the weather.

Dr. WALLIS observed, that it would vary with keeping; but Mr. HOOKE was of a different opinion.

New Baroscope.

Mr. HOOKE upon this occasion acquainted the President and society, that he had a baroscope making, which would make the alteration of the pressure of the air as evident, as should be desired; and that instead of two inches or thereabouts, which was the difference, that is usually observed between the highest and lowest altitude of the upper surface of the quicksilver above the lower, he could by this make that difference two feet, or two yards, or two fathoms, or more, if it should for any use be found necessary.

Mr. ABRAHAM HILL queried, from what cause it was, that the quicksilver and tube being well purged of air, and the experiment being made with great care, the quicksilver had been found to stand to the height of seventy inches, contrary to the received theory of the gravitation of the air.

To which Mr. HOOKE answered, that though by such diligence and care as he had mentioned, he had made the quicksilver to stand at that height; yet even then, if any considerable jog or shake were given to the tube, in which the quicksilver was suspended, the quicksilver would leave the top of the tube, and fall to the height of thirty inches, or thereabouts, according as the standard of the quicksilver was at that time: and he added, that this second suspense depended upon a second cause, which he had formerly explained in a discourse to the society.

Experiments Designed for Next Meeting.

The President inquiring what experiments were designed for the next meeting, Mr. HOOKE undertook to have the hydrostatical experiment ready; as also a further improvement of the microscope.

Pressure of Atmosphere.

Dec. 13. Mr. HOOKE explained the manner how the air at sometimes pressed more, sometimes less; and that was from the real access and accretion of vapours raised up in the form of air;

which, whilst intimately mixed with the air, might augment the bulk, as also the specific gravity of the air so long, till by a second sort of vapours all those others become precipitated or condensed into the form of water, whereby the air, out of which they are separated, become lighter in specie; and also the altitude of the aerial cylinder becomes lower: for the gravitation of any fluid upon an equal bottom of the containing vessel is always in a proportion compounded of the specific gravity of the fluid itself; and secondly, of the perpendicular altitude of the said gravitating fluids. This he affirmed he had made out formerly to the society by many experiments, as their Journal Book would show.

And whereas Mr. Hooke had read in the minutes of the last meeting, that he had contrived a barometer, by which an infinite number of small mutations of the air might be discovered, which would be wholly invisible, and insensible by the more common air-poises, the President advised him, that whatever the contrivance was, he should reduce it to a certain standard of inches and parts, as decimal, centesimal, or thousandth parts of inches.

Improved Single Microscope.

Mr. Hooke then showed the experiments appointed for this meeting; and the first was an improvement of the single microscopes, by which the little animals were exhibited much more magnified and very much plainer, though to some persons they seemed not so plain; the reason of which was to be ascribed to some otherwise imperceptible defects of the eye.

Water-poise.

His second experiment was of a water-poise, whereby the difference of the specific gravity of liquors was manifested by the sinking of the neck of a poise into the fluid examined, not at all making use of scales, as in the former experiments. And the niceness and curiosity of this instrument was shown to proceed from the very great difference between the bulk of the poise under the fluid, and the smallness of the sensible part of the neck, which might be made as one thousand, or ten thousand, or a hundred thousand to one; and consequently that the difference or alteration of the specific gravity of any liquor examined thereby might be discovered, though it were altered but 1,000, or 10,000, or 100,000 part of it.

Dr. Croone also urged, that this was the same with the common water-poise. But Mr. Hooke showed wherein it differed; namely, that whereas the most exact of that kind, that he had hitherto seen, would not distinguish to a much smaller quantity than about a hundredth part of the bulk, this would do to a hundred thousandth; which was a thousand more exact. And,

secondly, that whereas that did it only indeterminately, and without any respect to the specific gravity by uncertain divisions; this was designed to define and determine that also, by giving the proportion, that the said alteration had to the whole.

Hooke's Ideals.

This was the third instrument of this kind, which had been produced to the society by Mr. HOOKE. And the reason, which he gave of his so doing, was, that it was not his design or intention to load or trouble the society at their meetings with a confused enumeration of experiments of one kind, which might be made with every one of the said instrument; that being only the work of a labourer or operator to perform, when once the instruments were contrived, and the method chalked out. But his design was rather to improve and increase the distinguishing faculties of the senses, not only in order to reduce these things, which are already sensible to our organs unassisted, to number, weight, and measure, but also in order to the enlarging the limits of their power, so as to be able to do the same things in regions of matter hitherto inaccessible, impenetrable, and imperceptible by the senses unassisted. Because this, as it enlarges the empire of the senses, so it besieges and straightens the recesses of nature: and the use of these, well plied, though but by the hands of the common soldier, will in a short time force nature to yield even the most inaccessible fortress.

And of this kind were all those instruments, which he had since the last meeting of the society endeavoured to explain and show to them. Such were the microscopes, which he had there exhibited, which would as much exceed the common ones, as they did the naked eye; and consequently were an improvement of that sense, which is the most spiritual of all the five. Such were the water-poises newly explained: and such was the barometer, which he was now preparing; for that thereby multitudes of mutations of the pressure of the air, which were wholly imperceptible to the common barometer, would by it be discovered. And to show, that this was not purely conjectural, he affirmed, that by an instrument of this kind he had discovered such mutations and motions in the atmosphere, as were very surprising and very significant; such as the tremulous motion of the said barometer before a great storm ensuing, which could in no respect be assigned to any shaking of the house from wind, or the passage of carriages near the place, which was purposely taken notice of.

Atmospheric Pressure.

The experiment propounded by Mr. HOOKE for the next

meeting was in order to explain the phenomena of the gravitation of the air, and the differences thereof caused by the rising of vapours from the earth; concerning which he affirmed, that he had shown several experiments, and he supposed, that they were in the Register Books of the society: but notwithstanding there having been much debate concerning that matter, and several persons there present not being well satisfied concerning the manner, reasons, and signification thereof; it was judged convenient, that it should be prepared by the operator.

Performance of Experiments.

Dec. 19. It was ordered, that what experiments shall be undertaken by the curators shall be propounded a fortnight before the showing thereof, that objections, answers, and confirmations may be timely thought of: and

That the curators or any other person showing an experiment to the society shall explain the same, and show the design and usefulness of it.

Animals in Pepper-water Shown to Lord Halifax.

Dec. 20. The Vice-President desired, that the experiment of the small animals in pepper-water might be shown to the Lord HALIFAX, then present, who plainly discovered them swimming up and down in the liquor.

Hydrostatical Experiment.

Mr. HOOKE showed an hydrostatical experiment mentioned at the last meeting, which was in order to explain how the mutations of the baroscope were occasioned by the different pressure of the air; which pressure was sometimes greater, sometimes less, according as the exhalations or vapours raised up into the same augment the specific gravity of it, and the bulk also or perpendicular altitude thereof. This he made appear by means of a very high body of glass filled with water, into which was let down a standard of pressure made by a bended tube of glass, in which mercury was put; which, as it descended deeper into the water, and consequently the pressure increased, was raised on one leg, and depressed in the other leg thereof. Then a bladder was tied to the end of another glass cane, and by the breath, after it was sunk down into the water, was blown up; by which the fluid was increased, though not the specific gravity; and thereby the perpendicular altitude of the pressing fluid was increased, and consequently the pressure upon the mercury in the standard. The same was verified by a further trial made with a large bottle of quicksilver close stopped and let down into the body of the said water: and it was alleged by Mr. HOOKE, that the same effects would follow from whatever body it were,

that was thus put into the fluid, and augmented the bulk thereof, without at all altering the specific gravity of the same.

Dr. Croone remarked, that the same liquor, by being put into different cylinders, and so different postures, much augmented the pressure of the same quantity of fluid upon the respective bottoms.

But Mr. Hooke observed, that the same quantity of a fluid body, by being put into ever so much different cylinders, the whole pressure of the said fluid upon the whole bottom of the one would be equal to the whole pressure of the other upon the whole bottom of the second; because the gravity of the fluid must be the same in all postures, and the space possessed by the same would also be equal; for as the base of the bigger is to the base of the less, so the perpendicular altitude of the less is to the perpendicular altitude of the bigger.

To this Dr. Croone could not assent, but alleged, that he would make out the contrary by experiment.

Experiments Proposed.

The experiments propounded by Mr. Hooke for the next meeting were the prosecution of the hydrostatical experiment, and a further improvement of the microscope.

1677/8

Meteorological State of the Atmosphere.

Jan. 3. The minutes of December 20 were read; which occasioned much discourse concerning the constitution of the air as to its transparency and opacity, gravity and pressure, fogs, mists, rains and wind; concerning which, it was debated, whether an opaque, foggy, or misty air were heavier than a clear transparent air; and it was concluded, that the transparency or opacity of the air does not at all contribute to the gravity or pressure thereof; though on the other side the extraordinary gravity of it might sometimes be the cause of its opacity. And the reason was alleged, because sometimes, when the pressure of the air hath been greatest, it hath been observed, that the air hath been as transparent and clear as at any other time whatsoever; and at other times, when the air hath been exceedingly light, fogs and mists have been taken notice of: and that it was supposed, that the transparency and opacity of the air proceeded only from the uniformity of the parts of the air, and the opacity from the difformity and incongruity of them. That it was thus explained by Mr. Hooke, that the ether, which encompasses the earth, is the grand or universal menstruum, which dissolved takes up into itself, and suspends all sorts of vapours and exhala-

tions whatsoever; viz. all those bodies in the atmosphere, which make up or constitute that body, which hath a very great springiness in it; and which will not pervade the pores of glass, but can be confined and included by it, much after the same manner as water dissolves salt, sugar, or the like into itself, and keeps it suspended and intimately mixed with it, that so long as the vapours and exhalations remain thus dissolved and perfectly mixed and united with it, they appear perfectly transparent. But when, by the mixture of different sorts of vapours they either unite with them, and leave the ether; or those other being more congruous to the ether unite and coalesce with, and jostle out these, and so make them a distinct body, these vapours or exhalations become as it were opaque; that is, though really they are *in minimis* as transparent as formerly, yet by being disunited with the air, and having a different refraction, they make the air seem opaque and foggy. That these changes are often wrought from transparency to opacity, and from opacity to transparency; and yet the gravitation of the air not at all altered, by reason, that the same bodies remain suspended in the same part of the atmosphere; and consequently their gravitation cannot be at all taken away. And whereas the Vice-President objected, that what was alleged was but hypothetical; and that it was not very evident, that there was any such thing as an ether, much less was it understood what it was, and what properties it had; or that the air consisted of such parts, as was alleged; Mr. HOOKE answered, that by multitudes of experiments he could make it very evident, first, that there was such a body: secondly, what many of the properties of that body were: thirdly, how very considerable and powerful those properties were in producing multitudes of effects ascribed to other causes generally: fourthly, how those properties might be examined and assayed and reduced to a standard, viz. to number, weight, and measure; and consequently, that he could make it a subject fit to be further inquired into by the society, whose business it is to be directed by the great schoolmistress of reason, experience; and not to be ruled by groundless fancies and conceits.

By these ways he explained the phenomena of the great gravity of the air upon the long blowing of an eastwardly, and the lightness of it upon the blowing of a southwardly wind; the air in the one coming over a vast tract of land, and so taking up into itself great quantities of exhalations, which remain suspended and mixed with it by reason of their congruity; and the other blowing over a great space of sea, which affords a less quantity of parts disposed to make air.

He also further explained the reason of the ready converting of vapours into water by the cold of the air, those watery parts

being more easily precipitated or separated from the air by the want of heat to keep them agitated; as was instanced in the appearing of one's breath in cold weather, and the easy conversion of the wind produced by water heated in an aeolopile into water again by the want of that heat and agitation.

Wherefore the gravity of the air arising only from the quality of those gravitating parts, which were suspended in the form of air, the greater the one is, the greater also must consequently be the other.

Cherubin's Optics.

Mr. Hooke acquainted the society, that he had met with a discourse of optics newly published by Père Cherubin, containing descriptions of several sorts of binocular telescopes and microscopes; and of an instrument of taking the figure of things at a distance by the help of a telescope. He was ordered to procure that book for the society's library.

Jan. 5.

Letter from Hooke to Martin Lister.

These for Mr. Martin Lister at York.

Gresham Coll. Jan. 5. 1677/8.

Sr. Together with the favour of your letter I received your excellend Discourse which I presented to the Councell of the Royal Society and have obtained their license for printing the same, signed by our President the Rt. Hon. Sr. Joseph Williamson his Maj'ties principal secretary of State. But your paper about the Roman Pottery I cannot yet retrieve, though I have gotten a great number of our papers & letters from the executrixes custody. But we want most of our late discourses & letters, and they give us a very great deal of trouble to procure those we have. But however I shall indeavour it. I doubt not but you have heard of the changes that have been made in our Society. It hath Ile assure you very much revived us and put a new spirit in all our proceeding which I perswade myself will not only be beneficial and delightful to the members of the Society, but to the whole learned world.

Among the informations we received last week from foreign parts there was one which the Society upon the reading of it was pleased to order me to communicate to you, as being about the same subject whereof you treate. And that was a Observation of Swammerdams, thus worded in his letter:—Dum limacum

omnes genus qua possum diligentia scrutor, praeter omnem expectationem, et contra quam vel sapientissimus quisque somniaret in cochleam incidi viviparam et sub vulgari species, in effabilem sui creatoris potentiam representantem.

This with some other things he seems to be ready to print. Sr. I hope the death of Mr. Oldenburg will be noe occasion of your omitting to communicate to the Royal Society such other excellent observations and discourses as you shall for the future think them or the world worthy of. This I can assure you that they have a very great esteem and value for such as you have hitherto imparted, and I doubt not but what you shall for the future send them in that kind will very much oblige them. This you may either send to Mr. Martin as you did this last or els to Sr

Your very humble servt

ROBERT HOOKE.

You may direct your letters to my chamber in Gresham College.

The Ether.

Jan. 10. The minutes of the last meeting were read: upon which a discourse arose concerning the ether, which Mr. HOOKE affirmed to be the menstruum, vehicle, or most fluid part of the air, into which the exhalations or vapours, which compose the atmosphere, are dissolved or taken up, after the same manner as salt, sugar, or any other tingeing body, are dissolved or taken up by water or other dissolving liquors.

That the atmosphere or air, that gravitates on the quicksilver in the barometer, is only that part, which was thus dissolved and taken up; and that the other part or ether readily and freely pervades the parts of glass; whence glass becomes as it were a strainer to separate between the ether and the gross parts of air. That according to the quantity of these vapours or exhalations raised up and suspended thus by the ether in the atmosphere, so was the pressure of it upon the mercury in the barometer: that the rarefaction and condensation of these vapours did not at all alter the pressure, provided the same quantity of the said vapours were in both the said conditions the same.

The Vice-President doubted, whether there were any such thing as that ether, which Mr. HOOKE had hypothetically supposed; and said, that he would gladly see some experiment, that would make it evident, that there is such a body mixed with the air.

To this Mr. HOOKE replied, that he could by hundreds of experiments evidence the reality of such a body; and that from

these experiments he was able to collect the several properties of that body; and how many and how very considerable effects it produced in bodies. He further added, that he had a catalogue of such experiments, which he thought he should have occasion shortly to make in order to the elucidating a theory, which he designed to make public hereafter.

The Vice-President further inquired, whether the dryness or moisture of the air did not cause an alteration of the gravitating power upon the earth.

Mr. Hooke, in answer to this, affirmed, that the dryness or moisture of the air contributed not at all to the gravity or levity thereof; but only the greater or less quantity of the vapours held suspended by the ether in that form; and that whether it were in perfect air, or condensed into small globules of water, which yet remained suspended, it was the same thing, provided they remained suspended.

He further added an explication of what he meant by air said to be lighter or heavier *in specie*, viz. that that air, which had a greater quantity of exhalation in the same extension was the air, that was heavier *in specie*, and that, which had a less quantity, a lighter *in specie*: and that the condensation or rarefaction of the air added not at all, nor took from its gravity, the same quantity of exhalations in the whole cylinder.

The Vice-President inquired further, whether the air could be made heavier *in specie* by any other cause than cold? To which Mr. Hooke answered, that not only forcible condensation, but several fumes, smokes, and vapours, which may be raised up into the air, may produce that effect; but that it is very difficult to make it sensible by the barometer.

Barometric Experiments.

Jan. 17. Upon some further discourses about the barometer Mr. Hooke suggested, that he had already brought in and showed the society several of his experiments, in order to elucidate a theory, which he had on that subject; and that he had as yet divers others behind, which he designed, as fast as conveniently he could, to bring in likewise; and when they had been all shown, to set down the theory of that matter, as he had conceived it.

Sword-fish.

Mr. Hooke mentioned the relation, which he had received from Mr. Newland, of an accident which happened to a ship, wherein he was concerned, in its voyage from the Streight's mouth to Alicant, from a sword-fish, of which the society had received an account formerly.

Barometer.

Mr. HOOKE produced his experiment, in order to explain the pressure of the air upon the mercury in the barometer; which was a large tube of glass about three feet long sealed at one end, and opened at the other. This was filled with water pretty near the top: then a glass made of the form of an inverted siphon, and containing at the bottom a pretty quantity of mercury. This tube being gradually sunk down into the water, it was very obvious, how the pressure of the water upon the mercury in that part of the siphon, which was open to the water, depressed it, and raised the same in the other part, which was open to the air, and excluded the water. And it was plainly shown, that the cylinder of mercury kept up by the pressure of the water was always about a fourteenth part of the length of the water-cylinder between the surface of the quicksilver and the top of the water. It was further explained by him, and showed, that the air and water in this represented in all circumstances material in this trial the ether and air in the common barometer; that the mercury was common in both: for, as it was supposed, that the barometer at the top of the cane admitted the ether and excluded the air, so in this it admitted the air and excluded the water.

He further showed how the alteration of the specific gravity of the air alters the pressure, though the cylinder or altitude of the pressing atmosphere were the same: to make which more plain by an experiment, the fresh water, with which the great glass was filled, was poured out, and was filled with a very strong solution of salt, care being first taken to observe the exact comparative height of the mercurial cylinder to the fresh-water cylinder: and it now appeared very plain, that the same altitude of salt-water kept up a cylinder of mercury much higher than the fresh water.

Here by the way Mr. HOOKE showed how the pressure of the air decreased, as by ascending a mountain approach is made nearer to the top of the air. And he mentioned, that he had formerly brought in to the society divers discourses and experiments; by which he had shown, that the pressure of the air actually decreases in gravity, according as the experiment was made farther from the centre of the earth, after the same manner as the decrease of the pressure of the water was very visible in this instrument. He added, that he was preparing an instrument for some trials to that purpose, which he would shortly show. These were two other instruments of the like nature with those other, which he had produced since the last recess of the society: which instruments were not designed to show one or two single experiments and no more, but to be constant and standing instruments, whereby all the phenomena of gravitation and pressure may be explained by hundreds of experiments.

Further, when a query was made, why the longer end of the siphon was not sealed up, but remained open; which was otherwise in the barometer, Mr. HOOKE explained the same, and showed, that it was by reason, that the spring of the air, that was included, would vary its pressure upon the rising or falling of the cylinder of mercury, which being left open, the weight of the air did not any more than the other did in the barometer.

Upon this some queries being made concerning the spring of the air, Mr. HOOKE explained that theory, and showed, that, as the pressure of the water in the greater cylinder increased, it raised the mercury in the opposite leg; and as that rose, so the air thus included would be condensed into a less room, and consequently have so much the stronger spring.

Dr. WHISTLER objected, that though the air was more condensed, yet he conceived, that the spring of it did not increase proportionably; explaining his supposition by the instance of a fleece of wool, which would only spring so much, and no more.

To which Mr. HOOKE answered, that though that might seem a little to explain what is meant by the springiness of the air, yet it was no way fit to make out all the appearances of the springiness of it: for that, as he had also formerly proved in the society, the spring of the air is always condensible and rarefiable, which the wood is not: and that the force of the very spring is always proportionate to the condension of its bulk.

The President hereupon desired, that such an experiment might again be shown at the next meeting, which Mr. HOOKE promised to take care of and contrive accordingly. He promised likewise, at the President's desire, to show hereafter his theory of springs in general, having several years before showed it to the King.

Upon this several queries were made, whether the air near the earth were not of very different degrees of specific gravitation; and whether the pressure was the same. To which Mr. HOOKE answered, that the parts of the air, as to their expansion, vary very much, according to divers circumstances attending them, viz. pressure, heat, &c. That oftentimes the parts of the air near the earth might be much lighter *in specie* than those at a considerable height above it: that though in the experiments of rarefaction of the air, it were showed, that the higher parts must always gradually grow lighter and more rarefied, yet in the atmosphere it did not always happen so, but that sometime the rarefaction and condensation went *per saltus*, and by very great leaps. As an argument of this he urged the swimming of the clouds in the air; which seem to be upon the smooth surface of some fluid underneath them, all the under sides of them being perfectly defined, smooth, and horizontal; whereas all the upper

sides of them are undefined and in heaps. Hereupon Mr. HOOKE said, that he had an instrument for examining the specific gravity of the air, which was distinct from spring or pressure.

Dr. HOLDER mentioned, that somewhat like this was also to be observed in smoke, which will run along, and spread itself a great way, keeping as it were the same distance from the ground.

Dr. KING instanced, that a gentleman, who was a patient of his, could two or three miles off from London discover when he entered into the smoke of London.

Mr. HOOKE gave a reason, why in this experiment the mercury would of itself rise higher in a greater, and lower in a less pipe: and why water would do the quite contrary; which by the experiment then shown was plainly made appear to be so. He showed, that this was the same with what he had long before published.

He added, that these and all other experiments, that had been formerly made by the society for the examination of the nature and properties of the air, though they had been hitherto the opprobrium of the society from such persons, as thought themselves masters of all knowledge *a priori* and by revelation; and despised all such, as was acquired by experimental inquiry; yet there is no subject in nature more proper for the society's examination and exercise. For whatever may be said, that the weighing of the air, and the exhausting and condensing it in ether vessels, and the like, are trivial and impertinent tricks; yet he doubted not to make it evident, that an exact and thorough knowledge of that is of more concern to mankind than all the other physical knowledge in the world. For it is by that we continually subsist, and without it we cannot live one-tenth part of an hour. It is from that proceed the causes of infinite diseases; and it affords as many remedies for those distempers. It is that, in which we continually reside: it is the cause *sine qua non* of all vegetables and animals upon the land; and influences even the fish in the sea. Infinite and unspeakable are the uses of it to the husbandman, the merchant, the tradesman, the mechanic, &c. And that age will be deservedly famous, which shall perfect the theory of it.

Barometric Pressure.

Jan. 24. [Referring to M. PASCAL's observation that mercury in a barometer stands at a different height at the top and bottom of a mountain] Mr. HOOKE remarked, that this proportion of the two cylinders of mercury and air was not at all times alike, by reason that the specific gravity of the air alters from many causes; as does also the specific gravity of the mercury: but that by this instrument, which he was preparing, that inequality would be discovered.

Microscopic Appearances of Blood and Milk.

Mr. HOOKE produced two experiments, which he had promised at the last meeting, being those which were mentioned in the first part of Mr. LEEWENHOECK's letter;[1] viz. concerning the constituent parts of blood and milk; which were very plainly to be seen by making use of a small piece of looking-glass plate (instead of the usual foot of the microscope) which was very smooth and clear; and by spreading a little of the blood and milk on the top of it, and looking against the flame of a candle. From whence it appeared, that the blood consists of two substances, the one a containing liquor undetermined and undistinguishable as to its parts, flowing about and encompassing the other, which consists of an infinite number of exceedingly small parts, which were plainly perceived to be globular: all which parts were very equal as to bigness, and were seen upon the turning of the microscope to move to and fro very swiftly and very freely, they seeming to cross one another very much, and to move confusedly, though all tending the same way.

In milk the like substances were very visible, only with this difference, that whereas the globular parts of the blood were all of very equal magnitudes, those of milk were extremely different. These were exceedingly white like little pearls, whereas those of the blood were red.

Blood and Milk.

Jan. 31. Mr. HOOKE then exhibited the experiments of the last meeting, to show the great fluidity of one part of the blood and milk above the other; whereby it plainly appeared by the very free, swift, and confused motion of those exceedingly small globules through the body of the liquor, in which they swim, that it must be very fluid and yielding.

Campani's Inventions.

Mr. HOOKE produced likewise a book published by MATTHEUS CAMPANI, entitled *Horologium solo Naturae Motu Temporis momenta metiens, & Circinus sphaericus Lentibus poliendis, &c.* of which he gave the following account: that this writer, who was rector of a parochial church, and seemed from some passages in his book to be brother to that CAMPANI, who made glasses in Rome, endeavoured to make himself the author of two inventions, which had been long before published and shown to the society by one of their own members. The first, of two pendulums rectifying one another was shown by Mr. HOOKE, January 2, 1666/7, as appeared from the Journal, and from the testimony of many, who could not have forgotten it. The second, called by

[1] Read on Jan. 17.

CAMPANI *circinus sphaericus*, for making of glasses, was the same with that published by Mr. HOOKE in his *Micrographia*, in 1664; who did not doubt but that this pretended inventor was aware of it, since otherwise he would not have endeavoured to antedate it so much as he had by making it prior to October 6, 1664, citing a letter of Monsieur HUYGENS for his voucher, though the words quoted by him assert no such thing. But upon a perusal of the book it was plain, that CAMPANI could be the author of neither of the inventions, since he seemed not to understand either mathematics or mechanics enough to know, whether the things were true, when done; and therefore it was very improbable, that he was the inventor of either. Nor did he at all explain how either of the inventions may be performed either mathematically or mechanically, as any one upon perusal would easily find.

Generation of Creatures in Pepper-water and Rain-water.

Feb. 7. It was desired, that it should be tried, whether white pepper steeped in water would produce any small creatures.

Mr. HOOKE mentioned, that he had found great quantities of those worms in rain-water; and that he supposed them to be generated therein from small invisible creatures flying up and down in the air, after the same manner as other sorts of insects in the summer-time had been observed by Mr. HENSHAW to be bred in rain-water from gnats, and to be converted into them again at last: that he had observed them also in river-water and well-water, though not so plentifully: that though the pepper-water, in which these animals were swimming, were frozen into a lump of ice; yet letting it alone to thaw of itself he had found it again very full of those living worms, as if the frost had not done them any harm at all: that he had found also a sort of flat animals, which would contract and dilate their bodies somewhat like a leech; and that their motion in the water was different from that of any other creatures, which he had ever seen in the water, viz. a motion of writhing their bodies in the same manner as a board does, when it is said to be out of winding; and that thereby they guide themselves, and shoot through the water with great swiftness.

Relation of Pressure and Volume of Air.

Mr. HOOKE showed an experiment to prove the strength of the expansion of the air to be in proportion to the quantity of the air contained in the same space; so that half the quantity had half the strength, and double the quantity double the strength. This was showed by a glass cane sunk down into another filled almost to the top with quicksilver. The first was

open at both ends, but the other hermetically sealed at the bottom. In the first was left about three inches of its length filled with air, and the top of it was stopped with soft wax, that no air might enter in or go out through. Then this cane was lifted up so far, as till the air was expanded into twice its dimensions, and the quicksilver under it was observed to rise only to half the height of the standard. When by lifting it higher, it had acquired four times its first dimensions, the cylinder under it was found to rise but three-fourths of the standard; which plainly evidenced one part of the former theory, viz. that the force of the spring of the air was diminished in proportion to the expansion; and that half the quantity had but half the strength.

The other part of it was designed to be exhibited by another apparatus, which was made ready for this trial; but upon examination it was found, that a little part of the top of the glass for that trial was flown off; and therefore the experiment was deferred till the next meeting.

Force of Compressed Air.

Feb. 14. An experiment to prove the force of the compressed air was shown by Mr. HOOKE, by which it evidently appeared, that the force necessary to condense the air was always proportionate to the condensation. Which was verified by several trials of several degrees of condensation; that is, to condense the air twice, required twice the strength, and thrice, three times the strength, &c. that is, if the same quantity be condensed, then the force or weight of the power, that makes the condensation, shall always be reciprocal to the dimension: but if the dimension be the same, then the force shall be always proportional to the quantity of air contained in that space. This was experimented in a tube of glass about ten feet long; the one end of which was sealed up hermetically, and turned or bent back again in the form of a siphon reversed; in which reversed part there being left ten inches of air, quicksilver was poured into the other part, till the weight thereof had condensed the same into five inches space; the height of which cylinder of quicksilver was found to be about thirty inches. Then the pouring in of quicksilver was continued till the same air was condensed into two inches and a half, and the height of the cylindrical mercury was found to be ninety inches. And the same proportions were observed to be in other compressions.

Specific Gravity of Air.

Feb. 21. Mr. HOOKE produced an instrument to examine and show at all times the specific gravity of the air, in which it is placed, without any respect to the heat or cold, pressure or

spring of the air: but the said property of the air was not showed singly by any other instrument; nor was it proper or capable to show any other quality of the air, as some had thought, except only the specific gravity of the air. This instrument was first proposed by Mr. HOOKE to the society as appears from the Register Book: but the experiment and instrument itself was not before this time exhibited at the meetings of the society.

This instrument made to demonstrate the said property of the air was a very large and thin ball of glass sealed up hermetically. It was suspended at the end of an exact beam (which would easily turn either one way or the other) and was counterpoised by a small weight of lead or brass; but lead was best for that purpose. Then Mr. HOOKE explained the same, and showed the reason, why the ball would rise when the air, in which it hung, was heavier, and sink when it was lighter; and that it depended upon the same ground with the improvement of ARCHIMEDES' experiment by GHETALDUS.

He also explained the difference between the pressure and the specific gravity of the air. Whereupon Sir JOHN HOSKYNS added, that this was properly the barometer, and not the instrument so called.

Sir JOHN LOWTHER demanding, whether this instrument now produced was exact enough to make the small mutations in the air visible, Mr. HOOKE answered, that he did not exhibit this for any other use than to show the ground and a reason of the thing, and as a sensible object, upon which to reason and discourse, and for the more plain demonstration and explanation of all material doubts, that might arise; because without such a pattern or model of the thing designed, the propounder of such experiment or invention is for the most part not so readily understood, and very often mistaken or misapprehended by the auditors: nor can objections be pertinently made where the like model is wanting, especially in all mechanical subjects.

However, because in the experiment of weighing, that had been made, both the ball and the counterpoise had not hung in the same medium, it was desired, that an experiment should be shown to verify that assertion, which Mr. HOOKE promised to give order for against the next meeting.

Transit of Mercury.

Mr. HOOKE then showed the *Journal des Sçavans* in which was contained the observation of Mercury in the sun made by Monsieur GALLET at Avignon, the manner of which he in short explained to be very ingenious, proper, and accurate; and that seemed not to be defective in any material circumstance necessary

to be taken notice of in the observation: that Monsieur GALLET had remarked several immersions of Mercury, taking the declinations and right ascensions of it in every one of those places by a method very exact; and that thence he had deduced by trigonometrical calculation the longitude and latitude of Mercury in those several places, and the inclination of the orb and the true time of the conjunction: that he had taken notice, that the body of Mercury was oval, whose longest diameter was parallel to the equinoctial; and that at the emersion of it out of the eastern side of the sun it seemed to spread itself as it were upon the limb of that sun, appearing four times as big in diameter.

Experiments in Readiness.

Mr. HOOKE promised, beside the experiment of weighing air, to have ready some microscopical observation.

Air-poise.

Feb. 28. The minutes of the last meeting were read by Mr. HOOKE.

In prosecution of what was desired at the last meeting Mr. HOOKE showed an experiment to elucidate further the theory of the air poise produced at the preceding meeting, viz. to prove, that a large and very light ball of glass ordered, as it was at the last meeting, would, upon change of the specific gravity of the fluid, in which it was suspended, rise and fall with such a motion, as would make such alterations visible. In order to which, because such alterations were difficult to make in the air, and because the last instrument was only designed to show the gross mutations, and not the minuter ones, he having, as he affirmed, another way different from what was exhibited for producing that effect, which would be somewhat more chargeable to produce; therefore a glass sealed up as in the former experiment was suspended at the end of a beam, and so ordered, that a counterpoise to it, when both under the water, was hung into a vessel of fair water, which was taken notice of. Then into that water was put a small quantity of salt, that so by the dissolution thereof the water might be made heavier *in specie*, and thereby the bigger body, which was the sealed glass, ought to be made lighter than its counterpoise, every new fluid taking off so much more or less of the weight of the body contained in it, than was taken off by a former fluid, as a quantity of it equal in bulk to the said contained body weighs heavier or lighter than the like quantity of fluid, in which it was last suspended. Whereupon it was immediately very manifestly verified; for the glass ball immediately upon the putting in of the salt grew very sensibly lighter, and the counterpoise preponderated; whereby

all the objections and scruples, that were made concerning the former theory, were removed. And when inquiry was made, whether it could not be ordered so, as to make the minute variations more sensible, Mr. Hooke affirmed, that he had a way, by which he could make them as sensible, as should be desired; and that instead of varying an inch, he could make it vary ten, twelve, or more feet: The only inconvenience of which instrument was dust, because that settling upon the surface of the glass would augment the weight thereof: but for prevention of this, he said, that a glass cover and case might be so ordered, as to prevent all those inconveniences, and yet not at all hinder the air within from being sensible and compatible with the air without in the room, in which it was placed.

Mercury.

Mr. Hooke then produced an animadversion of Signor Cassini upon the observation of Monsieur Gallet of the passage of Mercury under the sun; wherein he compared that observation with those of Gassendus in 1631, and that of Mr. Hevelius in 1671, from which he made several conclusions concerning the motion of the node and the inclination of the plane of Mercury to that of the ecliptic.

Upon this occasion several reflections were made upon the observation of Monsieur Gallet of the oval figure of Mercury appearing in the sun. Mr. Henshaw supposed, that it might proceed from the refraction of an atmosphere about Mercury. Mr. Hooke conceived, that the body itself of Mercury might be of such a figure; and that it might proceed from the velocity of its whirling round upon its axis, he supposing, that the axis of its vertiginous or diurnal motion lies north and south, or at right angles with the seeming motion of it parallel to the : that a very swift vertiginous motion on that axis made the body of Mercury somewhat of the shape of a turnip or of a solid made by an ellipse turned round upon its shorter diameter; and he explained this hypothesis of his by the shape, that a hollow globe of glass will readily run into, if the pantillion or pipe, at the end of which it is fastened, be whirled round very swiftly. And the reason, which he conceived, why it must needs be turned round very swiftly, was on account of its nearness to the sun, whereby the superficial parts would be burnt, if it were not for the swiftness of its motion.

Aerial Telescope Without a Tube.

Here by the by Mr. Hooke explained his way, which he had formerly delivered to Mr. Oldenburg, of making use of glasses of any length without a tube: which was as follows:

A rope was so ordered, as to join the two ends together, and so to make a round rope. This was put through the pulley both at the top and bottom, and a large square or round board containing the object glass, which was fastened within a frame, so as to make it inclinable towards the eye, wherever posited; which was done by the means of certain strings fastened from the ends of the said board, and extended from it to the cell of the eye-glass.[1]

Transit of Mercury.

After this, upon a further discourse concerning the appearance of Mercury in the sun, there was much said concerning the penumbra of shadows cast by the body of the earth and by the atmosphere thereof; and it was explained by experiments and reasons, what part of the light of the moon in an eclipse, especially that of the darkened parts, was to be ascribed to the penumbra or partial light from the sun, and what to the light cast on it by the refraction of the same in the atmosphere: and it was shown, that that undefinedness of light or shadow, which was observable in eclipses of the moon, where part is shadowed, part enlightened, is to be ascribed to the penumbra or partial light of the sun: but that that light, which made the moon visible in a central eclipse, where no direct ray from the sun can come at it, must be ascribed to the refraction of the said rays by the atmosphere of the earth.

Microscopical Experiments.

Mr. HOOKE promised to produce at the next meeting some microscopical experiments, and particularly on some part of a muscle.

Air-poise.

Mar. 7. The minutes of the last meeting being read by Mr. HOOKE, upon the mention of the air poise, and the experiment, in order to prove it, of weighing two bodies in water, it was very much doubted; though by the experiment of weighing in water the instrument became sensible, so as to turn by putting

[1] An important part of HOOKE's Aerial Telescope is still preserved in the rooms of the Royal Society at Burlington House, together with two of his original sketches depicting its use. Astronomers have always been interested in the invention which is attributed by Professor R. SMITH to HUYGENS (*Optics*, 1738, p. 354), illustrating it with the well-known view, that has often been copied, of an observer with a lantern, 'to help him to see his object glass', which might be up on a pole 50 feet high. HUYGENS described the Aerial Telescope in *Astroscopia compendiaria tubi optici molimine liberata*, printed at the Hague in 1684. M. DE LA HIRE added a little machine for managing the object glass, *Mem. de l'Acad.* 1715, and SMITH, loc. cit., Fig. 616. These references were kindly suggested by Dr. KNOX SHAW.

a little salt in the water: yet whether the very small changes of gravity of the air could be made sensible by such an instrument, was still a question. Mr. HOOKE affirmed it, and that in order thereunto he would provide a glass blown large and light on purpose, and so order it, as that its variations or differences should be ten or twenty feet, if it were necessary; and consequently that it would be capable of discovering the smallest alterations of the air.

New Instrument for Investigating the Atmosphere.

He added, that he would shortly bring in a new instrument for the discovering of some properties of the atmosphere not yet known or taken notice of, and hitherto altogether insensible to us, though by the said instrument they would be made evident, and their natures discovered and determined.

Mercury.

Upon mentioning the particulars taken notice of by Monsieur GALLET, of the oval figure of Mercury, that subject was further discoursed of, and Mr. HOOKE's hypothesis was objected to, viz. that though such an oval figure would be caused by the whirling round of a fluid body; yet it was probable, that the body of Mercury is solid; and consequently this whirling could have no effect upon it. To which Mr. HOOKE answered, that though it might probably be now a solid body, yet that at the beginning it might have been fluid enough to receive that shape: and that though this supposition should not be granted, yet it was probable, that there might be about Mercury some fluid body, somewhat of the nature of the sea here upon the earth; and if this must be granted, it would be probable enough, that it would readily run into that shape, and make the same appearance: and that it is not improbable, but that the water here about the earth might do it in some measure, by the influence of the diurnal motion, which compounded with that of the moon he conceived to be the cause of the tides. But there were some other ways of explaining those appearances, which, when he had time, he designed to draw up in writing.

Long Telescopes Without Tubes.

Some objections having been made against the way of making use of long telescopes without tubes, Mr. HOOKE further explained the way, and answered all those objections, and particularly that of Sir JONAS MOORE, who supposed it only theory, and that it had never been practised or made use of, Mr. HOOKE affirming, that he had done it, and found it convenient enough in a glass of twenty-eight feet; and therefore he conceived, that

it might be as conveniently practised in a glass of any other length.

The Vice-President likewise moved, that the apparatus might be prepared, in order to show the experiment of it at some meeting of the society.

Penumbra in Eclipses.

Upon a further discourse about the penumbra in eclipses, the Vice-President said, that Mr. HEVELIUS had affirmed that penumbra to arise from the atmosphere, about the earth. But Mr. HOOKE asserted that to be a mistake, since the penumbra visible in eclipses of the moon proceeds from nothing else than the partial enlightening of such parts of the moon, as were in the penumbra by the light of the sun; for those parts, which are in the middle of the penumbra, are enlightened but by one half of the sun, the other half being shadowed by the body of the earth, which by an experiment with the candle, and an explication of schemes, was made so plain, that there were no further objections made against it.

Tendon of Muscle.

Mr. HOOKE attempted to show a microscopical experiment of the exceeding smallness of the parts of the tendon of a muscle, viz. that it was not above a twentieth part of the bigness of a hair in diameter; and consequently that a hair was four hundred times bigger than one of these. But upon making trial thereof, the tendon was grown so dry, that those minute parts could not be discovered, though the same were plainly visible in the morning. It was desired therefore, that the apparatus should be provided against the next meeting; and that the tendon should be provided fresh and fit for this experiment.

Micro-organisms.

Mr. HOOKE then showed several sorts of animals, produced in the steeping of aniseeds, coffee, &c. in water. These were much smaller than those of pepper, and had a quite contrary motion.

Mercury.

Mar. 14. Upon reading the minutes of the last meeting, wherein it was supposed, that the oval figure of Mercury might be caused by the velocity of its turbinated or diurnal motion upon its own axis, Mr. HENSHAW objected, that if it were so, why did it not always appear of that oval figure, when it was seen at its greatest elongation, as well as when it was in conjunction with the sun? To which Sir JONAS MOORE added, that Mr. FLAMSTEAD was of opinion, that it was caused by the refraction or defect of the glass, and proceeded from mistake, and

not from any real appearance, since he could easily make it appear so with any glass: and that there were many other mistakes in the said observation, which therefore could not be relied upon.

Mr. HOOKE answered to both these objections, that the figure of Mercury might really be always so oval, as Monsieur GALLET had affirmed that he observed it; and yet through the inadvertency of others it might not be taken notice of, and that possibly for want of as good glasses as his, it might not have been visible before. To confirm which, Mr. HOOKE said, that Monsieur GALLET had noted, that even in this observation through the three-foot glass it appeared round. Secondly, that the reasons, why it may not appear oval in its greatest elongation, may be three. 1. That it is very small and far distant, and very bright, all which circumstances make it very difficult, unless with very good glasses, to see any figure at all of its body, but only a radiating point. 2. That being seen only part enlightened, and never in opposition to the sun, the whole surface of it seen by us is never all en lightened, but only some parts of it; which is a sufficient reason to make it appear round, though the body of it be really oval, as is supposed; a part of this oval being really not seen, whereby the oval is changed into a circle. For it must be a very good glass, by which one is able to discover the true figure of Mercury, when half enlightened, by reason of its smallness and radiation; and it ought to be a very much better one to discover the figure of Mercury. 3. The figure of a dark body in a light medium is much better discovered than the figure of a light body in a dark medium, by reason of the radiation.

Next, as to Mr. FLAMSTEAD's supposition, Mr. HOOKE said, that it was no ways probable, that a man, who had made the whole observations with so much care, and with so many witnesses, and besides had so ingeniously and knowingly contrived the apparatus for observing it, could either be deceived himself, or endeavour to deceive others; and therefore, till there were better arguments than conjectures or hypotheses produced against these circumstances of the observation, it ought not to be rejected or condemned.

Long Telescopes.

Upon the mention of using long telescopes without tubes, several objections were made, as the bending of the lines, the difficulty of raising and fixing it, and the like. But Mr. HOOKE affirmed, that he had actually done it: and that he had tried Mr. HEVELIUS's sixty-foot glass without a tube, though it were afterwards tried also with a tube by Mr. COCK at Mr. OLDENBURG's desire. (See figure on p. 197 and text on p. 280.)

Hereupon it was desired, that a trial might be made of it with

a pole of ten feet long, that thereby the society might be satisfied of the practicableness thereof; which Mr. HOOKE undertook to do.

Penumbra.

Several discourses were made likewise about the penumbra, the result of which was, that it was concluded to arise from the partial enlightening of the parts in the penumbra, some parts of the luminous body being hid from them, whilst they were shined upon by other.

Micro-organisms.

Mr. HOOKE read a discourse of his own, being an account of his observations, which in prosecution of Mr. LEEWENHOECK'S discoveries he had made of the small worms in pepper-water, and in the steeping of several other liquors, as of barley, wheat, oats, aniseeds, coffee, &c. as also of sugar, alum, blood, milk, fat, ligaments, muscles, &c. and together herewith he discovered the several ways and contrivances by which he made those observations; and therein showed, how easily and apt such persons are to be deceived by the appearances of these transparent bodies through a microscope, who are not aware of certain properties of transparent bodies, especially such as are peculiar to substances of such small bulk. And for the avoiding and preventing all these inconveniences, he showed several ways and expedients, without which no true discovery could be made, and by the help of them they were very easily made. Some of those mentioned by him were glass plates, and plates of Muscovy glass, particular kinds of light, the immersing the bodies in waters and other liquors, the squeezing bodies between two glass plates, the stretching and squeezing others with a kind of tongues, &c. whilst they are looked upon in a convenient light by the eye.

Two Microscopes Compared.

After which he showed the method, by which he made two sorts of microscopes, and the conveniences and inconveniences of both these.

The first was a single microscope made by a small globule of glass, by means of which, with very little or no difficulty, any object might be prodigiously magnified.

He also explained how the globule was made out of a thread of glass, and how that glass thread and small glass canes were made.

The second was a double microscope consisting of two glasses, whereby many observations might be more conveniently made than with the single one. He then explained how, by the help

of these, the parts of a muscle, fibre, tendon, ligament, &c. might be examined: and to verify this by experiment, he produced a small part of the ligament of the neck of a sheep, and showed it to consist of an infinite number of exceedingly small threads, four hundred of which would scarce make the bigness of one single hair of a man's head. But as to the fibres of a muscle, he affirmed them to be very different, which he would some other time produce.

Deep-sea Trials.

Mar. 21. A discourse arose about more exact ways of making trials in the depths of the sea and other deep places. And Mr. HOOKE affirmed, that he had a way to examine the pressure to any depth with the greatest ease imaginable; and that was by a cane of glass, with which there had been several trials made near Sheerness, of which there is an account in the Registers of the society. That he had also other ways for examining the heat and cold of those submarine regions; and to fetch up the water from any depth; as also the earth, sand, &c. from the bottom: that he would give directions for the making of such an apparatus, if there were proper persons appointed to make trial with them.

Hereupon mention was made of great depths of waters within land, as of that of the lake of Geneva many hundred fathoms deep.

Coagulation of Tallow and Crystallization of Sugar.

Mr. HOOKE showed in the microscopes the manner how tallow coagulates, in order to explain the expansion of water upon freezing: and

The manner of the shooting of sugar dissolved in water upon the glass plate of the microscope.

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Deep-sea Trials.

Mar. 28. Upon reading the minutes of the last meeting, a discourse was occasioned concerning the ways then spoken of for sounding the depth of the sea by the help of a long pipe of glass, the lower end of which was perfectly sealed up hermetically, and the upper end so ordered, as upon its descending to admit of the water to enter in, according as the pressure of the water was greater and stronger upon the enclosed air; and upon pulling up again of the same pipe, and so as the pressure of the water decreasing, the air expanded itself, and found its way out without forcing out again the water, which had been admitted in its descent.

Sir CHRISTOPHER WREN objected to this, that the compression of the air might be occasioned by the cold as well as the pressure of the water; and so it could not be distinguished which part of the admitted air was to be ascribed to the cold, and which to the pressure.

Mr. HOOKE answered to this, that it was necessary, that there should be other instruments let down with the said pipe, in order to find the degree of coldness in the water at several depths below the surface; and that the said experiment was not less instructive than the other: for the performing of which he alleged that he had a contrivance, by which the same might be certainly examined, and thence the degrees both of the one and the other might be defined.

Some other objections were made, that the depth might be examined by the help of a line, as well as by the degree of pressure. To which it was answered, that as for the mere finding the depth of the sea, the line and plummet were not sufficient, because in great depths the line would be buoyant, and so not be strained straight: and where there was a motion of the water, the line would be often much sloped, and sometimes much bended. But besides the examination of the depth, this tube served also to inquire into the degree of pressure at certain depths: which was another useful inquiry, and likewise helpful for the finding the qualities of those lower regions as to heat and cold; which was a third inquiry worthy to be examined.

Structure of Muscle Fibres.

Mr. HOOKE showed an observation of the figure of the small and imperceptible parts of a muscle, which he had discovered by the help of a microscope. The muscle, which he had made choice of for examination, was that of a lobster's claw, the fabric of which was such, that all the motion must necessarily be made in the fibrous part thereof; since first the tendon is nothing else but a bone, and so not capable of shrinking or stretching; and secondly, the other end thereof is fastened immediately to the inside of the shell.

In this observation notice was taken, that the small fibres sought for, though as much magnified and enlightened as was necessary, did not appear till by the adding a small drop of water the irregular refractions on the outside of the fibre were removed; after which it was very plainly visible, that the whole fibrous part of the muscle examined consisted of an indefinite number of exceeding small strings extended straight between the inside of the shell and the tendinous bone in the middle; which were so small, that five hundred of them would scarce exceed the bigness of an hair.

Each of these small fibres or strings was conceived to be seen of the shape and figure of a wreathed pillar, or a stick naturally grown wreathed by the twisting of a string of ivy. Others supposed it of other shapes. But the determination thereof was left till another time.

Deep-sea Soundings.

Apr. 4. The minutes of the last meeting being read, Mr. HENSHAW, upon the mention of the ways of sounding the depth of the sea, asserted, that in Cardinal CUSANUS there was another way of sounding it, different from those which had been described by Mr. HOOKE, both by the help of a pipe, and also by the descent of a ball sunk by a weight; which weight leaving the ball when it came to the bottom, the ball ascended with the same velocity upwards, with which the weight and ball together descended to the bottom.

Hereupon Mr. HOOKE explained the peculiar contrivance of the application of the leaden weight to the ball, which he had invented and made use of, being the same, which was entered in the society's Register. As also he showed the uncertainty and inconvenience of the other contrivance, he having experimentally found, that it would often fail of performing the desired effect, either by leaving each other before they came to the bottom, or not separating at all when they came thither.

The President, Mr. HENSHAW, and Mr. HILL made several objections against the way of sounding the depth of the sea by the aforesaid contrivance with a ball sunk by a weight: the chief of which were founded upon the supposition of GALILEO, that descending bodies accelerate their motion continually in a duplicate proportion to the time of their descent; and therefore it seemed hard to conceive, how the theory propounded by Mr. HOOKE would hold true, viz. that the time of the descent and ascent of the ball is always in the same proportion with the depth of the sea, be it more or less, provided it were about two fathoms deep. The reason of which he alleged to be, that by passing about two fathoms in the water, the ball both in ascending and descending would arrive to its greatest degree of velocity.

Law of Falling Bodies.

The President further urged, that GALILEO, GASSENDUS, and MERSENNUS had all affirmed the same thing, that all descending bodies accelerate their motion in proportion to the squares of the times of their continued descent: and that they had, upon this supposition, been at the trouble to calculate the time, that a body would spend in descending to the centre.

Mr. HOOKE answered, that those calculations had been made upon a theory, and not upon experiment; for that experiment would evidence the contrary. And though in a vacuity of water, air, or any other gross fluid, those proportions would hold very near; yet in a medium, wherein there was a resisting fluid body, it would not hold in any wise, especially in those, which had a considerable proportion of specific gravity to that of the descending body. Hence he said appeared the reason, why a down feather being let fall in the air would descend therein, if it were not disturbed, by an equal degree of velocity. But on the contrary, if the said feather were let fall in a medium, whence all the air was exhausted, and nothing but a fluid ether left, he affirmed, that it would fall therein, as to sense, with the same accelerated velocity, that a stone would do in the open air. This, he observed, he had formerly showed to His Majesty at Whitehall. But that upon admitting the air into the space, through which the feather was to descend, it was plainly seen to descend with an equal degree of velocity the whole space, which was all the way very slow. He added, that *in vacuo* the descent of all bodies was equally swift, increasing continually its velocity by a duplicate proportion to the time of continuance; but that in all gravitating mediums somewhat of that proportion is impeded. Hence he affirmed, that in the experiments tried from the top of St. Paul's steeple it was very plainly visible, that a leaden ball would descend faster than one of the same bigness of wood, and that of wood faster than one of cork; insomuch that the heaviest would in that descent get near thirty feet before the other to the bottom. He further added, that even of bullets of the same substance the bigger would manifestly outrun the less in their descent.

As also, that all mediums whatsoever had some resistance to the motion of bodies through them, and that even those, which had least, had yet a very considerable opposition to a motion, that was proportionably accelerated. Hence it was, that birds were able to sustain themselves in the air; and that one might break the strongest oar by swiftly striking it against the water.

And further, that in the thinnest medium, though the acceleration were pretty near what was supposed by the aforesaid authors; yet that it was in none mathematically true, but that there would be in all mediums a certain degree of velocity, which the same descending body would never exceed, though other descending bodies might, and some others would never arrive to: after which degree was attained, the progress of the body would always be made by equal spaces in equal times, though ever so far continued, provided the gravitating powers remained the same.

Compression of Water.

Mr. HOOKE showed an experiment of the compression of the water in a glass pipe, in order to the exhibiting the experiment of examining the depth of the sea. And it was very visible, that the effect answered to what was asserted concerning it, viz. that the compression was proportionate to the depth of the glass below the surface.

Falling Bodies.

Apr. 18. It was then moved, that some experiments should be made at the column[1] on Fish Street Hill, of the velocity of the descent of heavy bodies, and what the resistance of the air is to that motion.

Mr. HOOKE affirmed, that he had a design to make several experiments concerning that and other matters at that place; of which he would give the society an account; as he had formerly done of those made at St. Paul's before the fire of London. He took notice, that there were in RICCIOLUS's *Almagestum Novum* a great number of such experiments made at Bologna at the tower of the Asinels.

Projectiles.

Mr. HOOKE moved, that for the examination of the descent of heavy bodies trial should be made with granados shot directly upwards, as near as might be, at Blackheath; in which trials observation should be made of the time of their ascent and descent; which might be very easily done, this being very visible at a distance.

Muscle Fibres.

Mr. HOOKE showed the microscopical figure of the fibres of a muscle, and explained the reason of their motion by a wreathed helical gut and a straight string, whereby upon blowing up the gut, and filling it with wind, the string became helical and shortened, being twisted about the gut: but when the gut was suffered to empty itself of the air, the string lengthened and became straight, and the gut twisted about it in a helical figure.

Mr. HOOKE proposed also an experiment for the next meeting, to show how the motion of the muscle might be explained, supposing the said fibres to be (like a necklace of hollow glass beads, which it represented) a string of small bladders joined together by the necks.

Apr. 25. A discourse was occasioned concerning the motion and fabric of muscles. And Mr. HOOKE showed an experiment in order to the explanation thereof, which was a chain of small

[1] The Monument.

bladders fastened together, so as that by one pipe the whole series might be filled; which they would be successively, one after another, that, which was next the pipe, being first filled, and then the next successively. Now it was supposed, that the globules of the fibres of the muscles, which seemed like a necklace of pearl, might be some fabric, as this of bladders, in which might be included a certain portion of air or other very agile matter; which air being included in so exceedingly small and very thin skins, was very easily wrought on by heat and cold, and other agitating properties of the liquors, that pass between them; and thereby they might be presently filled by the said included air being rarefied and emptied by the condensation of the same from the want of that heat continued: and so by the successive rarefaction and condensation of the same air included in the aforesaid chain of bladders the string thereof was made either shorter or longer, each of which was so much the more, by how the rarefaction or the condensation was the greater.

Upon this an occasion was taken, to discourse of the causes of the motion of the muscles; and how far the air taken in by the lungs might contribute towards muscular motion. And it was thought, that it was of great necessity for that very purpose.

Then the discourse of muscular motion was further prosecuted: and it was supposed, that those chains of globules might be filled with other liquors as well as with air. But Mr. HOOKE alleged, that the spirit of wine and divers other spirituous liquors were pretty suddenly susceptible of the degrees of heat and cold; yet in comparison with the exceeding sensibility of the air they were very slow and dull: and in order to explain this he promised to produce at the next meeting a glass, which should experimentally verify it.

Sir JOHN HOSKYNS objected, that the motion of the muscle could not be from the swelling or shrinking of the air; for that Dr. GODDARD had, by an experiment made in a vessel of tin, in which a man's arm was included, proved, that the arm took up no more room in the water, when the muscles were intented and made use of to pull, than when they were suffered to lie still without straining. To which Mr. HOOKE answered, that that experiment was not at all sufficient to prove or disprove the swelling or shrinking of the muscle; for that there being always some muscles, which counterbalance the other, and that as much as the one swells, the other shrinks; and so the same space is always filled by the two antagonist muscles together.

[It is noteworthy that Dr. CROONE, in whose name his widow, Lady Sadleir, founded the celebrated lectures on the Nature and Laws of Muscular Motion, was present but did not take part in the discussion of HOOKE's paper. R.G.]

Heart Muscle Contracting after Death.

Mr. HOOKE affirmed, that he had observed the heart of a monk-fish to beat many hours after it was cut out of the body.

Respiration.

May. 2. Upon occasion of a discourse about respiration, Dr. CROONE mentioned an experiment made by Dr. MERRET of keeping young puppies a long time without respiration, by suffering them to remain in their secundine after they were taken out of the dam. This observation was confirmed by Mr. HOOKE from his own trial; and he added, that he had found the heart of one of them beat the next morning, which he had taken from the dam the night before.

Structure of Lungs.

Upon this occasion much was discoursed about the fabric of the lungs, and how the air might be clean drawn out of them by the wind-pump, though it could not be expressed out of them by pressing them without breaking the little bladders.

Mr. HOOKE explained the reason, why this effect might be performed the one way and not the other: and that was, that the little bladders, out of which the lungs were composed, were joined to the branches of the aspera arteria like leaves to the sprig of a tree: that the holes, by which the aspera arteria and each of these bladders communicated, opened and closed according as the bladders were more or less expanded: but that they became perfectly closed up, when the bladders were not bigger than such a determinate magnitude.

It was discoursed, that the specifical use of the air or respiration was difficult to guess it; for some experiments had proved, that there might be a circulation without the motion of the lungs; and that a man might be stifled, though he moved his lungs and breathed, if it were not fresh air. This was thought a good argument to prove what Mr. HOOKE had asserted, that air was the pabulum of the animal spirits, and that, which was the principle cause both of the heat and animal motion; for that the blood was in the lungs both impregnated with fresh air, and so received an enlivening florid arterial colour, and also discharged great quantity of steams and fuliginous matter, that was contained in it.

Healing of Wounds.

Hereupon it was suggested by Mr. HOOKE, that probably the healing of plasters might be from nothing else than the keeping of the air from preying upon the tender wounded part, and from keeping in the moisture to keep it tender and supple.

The use of the skin of an egg for healing of fresh wounds was also mentioned: as likewise that this was the reason, why a dog heals his wound by licking it, keeping it thereby clean and moist.

An experiment was exhibited, to show how exceedingly sensible the air is of the alteration of the degrees of heat and cold. And it was made use of to explain how the motion of the muscles might be effected, supposing them to consist of an infinite number of such small bladders strung together, as had been shown at the last meeting by the microscope in the muscle of a crab.

Chelsea College.

May 4. It was ordered, that Mr. HOOKE treat with Mr. LEM concerning Chelsea College, and give an account of his proceedings at the next meeting of the council: And

That Mr. HENSHAW, Mr. HILL, and Mr. HOOKE, be desired to go to Chelsea College, and to get a survey of it sometime before the Thursday sevennight following.

Gresham College.

May 30. It was ordered that Sir J. HOSKYNS, Mr. HENSHAW, Mr. HILL, Dr. GREW, Mr. HOOKE or any two of them be a committee to consider of the best way of disposing of Gresham College; and that they have full power to treat and agree concerning the disposal of the said interest to the best advantage of the Society in their opinion: and that Mr. HOOKE do from time to time appoint the meetings of the said committee, and accordingly send notice thereof to the members of it.

Pressure of Air at Top and Bottom of the Monument.

Mr. HOOKE then gave an account of some experiments, which he made with Mr. HUNT and Mr. CRAWLY at the pillar on Fish Street Hill, concerning the difference of the pressure of the air at the top and bottom of the column and at several intermediate stations: and he affirmed; that he had found the quicksilver in the tube to stand higher at the bottom of the column than at the top of it by near a third part of an inch; and that he had observed the same to ascend by degrees, as near as he could perceive, proportional to the spaces descended in going down from the top of the column to the bottom: but because the said stations of the mercury were different from one another but very little, and so it was not easy to determine the certain proportions of the one to the other; therefore he proposed against the next meeting an experiment to be tried at the same place with an instrument, which would determine that distance a hundred times more exactly: which instrument also he there

produced, in order to explain the manner thereof, it being made upon the same principle with the wheel barometer, but more curiously wrought.

The experiments with his instrument were appointed to be tried at the column upon the Thursday following at eleven o'clock in the morning.

Salary.

June 6. It was ordered, that the Treasurer pay to Mr. HOOKE the sum of forty-five pounds for his allowance as curator of the experiments of the society for a year and a half ending the 24th of that instant June.

Transudation of Water through Stomach.

Dr. CROONE mentioned the transudation of water through the coats of the stomach, through which yet the air, which was more subtile, would not pass.

Mr. HOOKE added, that the reason of this is from the congruity of the coats of the stomach to water, and their incongruity to air, of which there had been many experiments shown to the society.

He further added, that there are very different salts; some, that are aerial, and have a congruity with the air, and would therefore easily mingle therewith, and leave the water: and there were other salts, that have a greater congruity to water and other aqueous liquors. And of this nature seem to be volatile or urinous salts, which are readily taken up by the air, especially if the air be assisted by heat. So that if there were a way of rendering sea-salt so volatile, the salt might be separated by sublimation from the water, as well as the water is now sublimated or distilled from the salt.

Separation of Fresh- from Salt-water.

Sir CHRISTOPHER WREN mentioned, that possibly there might be a way of separating fresh water from salt water, by suffering it to stand and settle in vessels; for that it had been observed, that the top of the water was very much fresher than the bottom; and that by pouring fresh water upon salt, it would remain a good while fresh at the top before the salt would rise thither.

This was seconded by Mr. HOOKE, who affirmed, that he had upon that principle made several weather-glasses, viz. by poising certain bubbles or bottles of glass sealed up hermetically, so as just to sink in fresh water: and that by this means he had found, that by putting salt into such water, or rather gently pouring water upon salt, and putting in the said poised bubble, the same would not sink to the bottom, but remain suspended in the body of the water, at first pretty near the bottom; but at

length it would rise higher, according as the salt more and more dissolved in the water, so as at last to float almost at the very top.

Saline Waters.

Dr. CROONE related an experiment of his own trial, which was, that by putting a crust of bread on the top of salt water, and pouring fresh water upon it he found, that they remained a long time distinct without mingling one with the other.

Mr. HOOKE supposed, that there might be a kind of precipitation or rather fixation of the salt out of the brine, by the straining through the sand; it finding therein somewhat of such a nature, that might mix with it, after the same manner as oil of tartar doth with oil of vitriol; from the colluctation of which might be produced a kind of salt perfectly insipid. And to make this the more probable, he related it as a known observation at the salt-urns, that the boiling of the aforesaid brine (which had been made by the evaporation of sea-water by the heat of the sun in the brine-pans) constantly separated from the same (though perfectly clear when put into the square iron boiler) great quantities of pure insipid white sand at the four corners of the said boiler. That the reason of this separation here he supposed to be the avolation of that volatile salt, which kept the said sandy substance dissolved and floating in the brine: that there was such an avolation of volatile salt, he argued from the strong smell of spirit of salt in the boiling-house.

Density of Air.

Mr. HOOKE mentioned an experiment for examining the gravitation of the air at several heights above the surface of the earth.

Duke of Norfolk's Library.

June 13. It was ordered, that Mr. HALL and Mr. HOOKE attend the Duke of Norfolk, and entreat him from the society, that since his grace was then pulling down his house, he would be pleased to suffer the library, which he had bestowed on the society, to be removed to Gresham College, where there was a room provided for it; and that they also deliver to the Duke the catalogue of that library.

Condensation of Vapours out of the Air.

June 20. Mr. HOOKE related an observation of the like nature, which he had made in the year 1665, in a deep well of one Mr. CLARKE near Banstead Downs, of three hundred feet depth: into which having, in the time of a very great frost and exceeding cold wind, which happened about Christmas, let down a bottle with spirit of wine so cooled by the air above, the same, when

pulled up again, appeared all covered over with great drops of dew: besides which a great many drops of water were observed to be run off from the bottle into the scale, in which it stood: which by him was attributed wholly to the warmth and vapourousness of the air at the bottom of the well and the exceeding coldness of the bottle let down, which condensed the vapours of that air into water.

Mr. HENSHAW and Dr. CROUNE mentioned the reeking of well-water in frosty weather, which was attributed to the warmth of the water and coldness of the air.

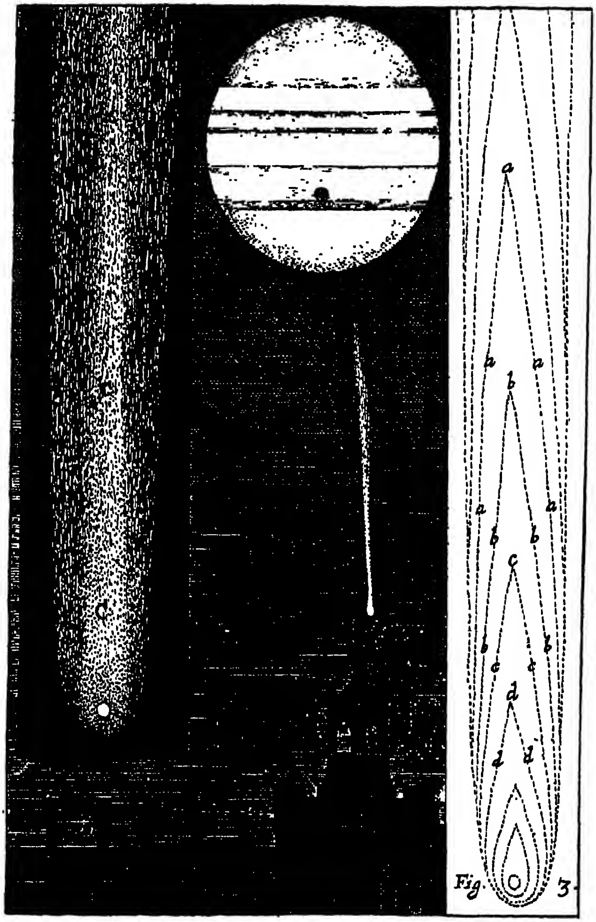
Mr. HOOKE conceived, that this was occasioned partly by the coldness of the air condensing the steams, which continually rise not only from well-water, but likewise from all other water, when of such a degree of heat; insomuch that all water exposed to the open air, when kept in such a degree of heat, evaporates such a quantity thereof in the space of an hour, though it scarce becomes visible unless in very cold or very hot weather; in very cold weather by the condensation of the steams into a mist; and in very hot by the playing or dancing of the air, as we commonly call the undulation of the rising vapours over rivers.

This also was partly to be ascribed to the keeping in of the vapours of the water in wells by the air of it; which being once satiated would take up no more vapours into it: whence as soon as this water was exposed to fresh air, that was unsatiated, the vapours were taken up more copiously by it. This sufficiently appeared from the damps of wells.

June 22.

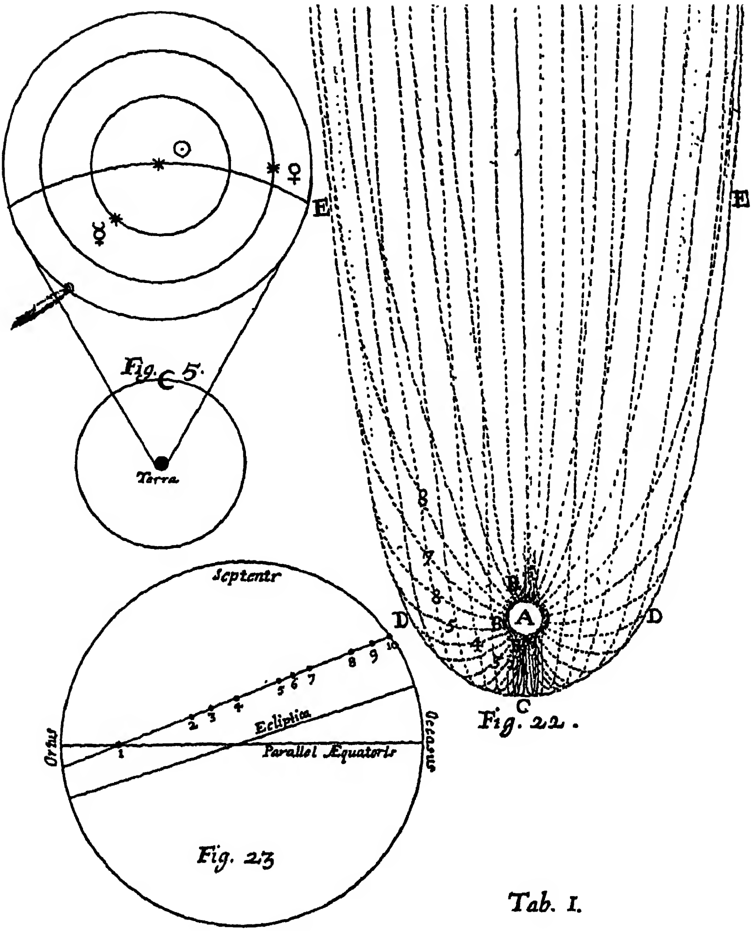
(27) *Lectures and Collections, Cometa and Microscopium.*

Cutlerian Lectures, No. 6, 1678.
Notice in *Phil. Trans.*, No. 139, p. 986, June 1678.



THE COMET OF 1677.

FIG. 1. Naked eye view of Comet when first seen on Saturday April 21, 1677.
FIG. 2. Telescopic view of Comet. This figure differs from that of M. HEVELIUS in respect of the parabolick termination.
FIG. 3. Prickt lines expressing the diminution of the blaze of the Comet.



HOOKE'S DIAGRAM FOR HIS CUTLERIAN LECTURE ON THE COMET.

Cock's New Microscope.

June 27. Mr. HOOKE then produced a new microscope made after his directions by Mr. CHRISTOPHER COCK, whereby the objects were exceedingly magnified, and yet appeared very clear. This was viewed by most of the members present with great satisfaction; and the microscope was ordered to be bought of Mr. COCK for the society's use.

Growth of Plants.

July 4. Mr. HOOKE supposed, that no plant whatsoever would

grow *in vacuo*, howsoever ordered: and further, that they would not grow very long in a glass perfectly stopped, though it contained both water and air, provided it had no communication with the outward air to refresh and renew the air: that a plant would be stifled in the manner of an animal, though not altogether so suddenly: but that length of time would make them grow pale, and sick, and die.

Mr. Hooke mentioned an observation, which he had several times made, that some of those plants, which had been set to grow in glasses of water, would after a certain time begin to pine and waste, and at length be all over covered with small insects; which in a short time would all be gone, and leave nothing but their husks behind, sticking all over the surface of the plant; and at the same time also the plant perfectly dead and withered, as if the plant had been nothing else but the nurse or dam of those insects, and that the spirit or life of the plant had flown away in the insects and had only lasted till it had brought forth that living animal offspring. He also mentioned, that he had observed several other things in plants of the like nature, which seemed to hint some such theory.

Change of Mushrooms into Worms.

Dr. Pell hereupon called to mind a certain fungus formerly given by him to the repository, which in a short space was all converted into worms, nothing of the mushroom being left but a little dust.

This was confirmed by Mr. Hooke, who had taken notice of that strange metamorphosis.

Plants Grown in Vacuo.

July 11. Mr. Hooke gave an account of the trial of an experiment propounded at the last meeting by Dr. Croone to be made to see, whether a plant set in a glass of water will grow *in vacuo*; and he related, that a plant being put into water upon the Tuesday preceding, by Mr. Hunt, and set into the receiver of the air-pump, he had endeavoured to keep the air exhausted ever since by now and then drawing out by pumping what air might have gotten in; and that the plant, which was a blade of mint, was observed to be withered and dead.

Slime of Eels.

Dr. Cox queried about the slime of eels, whence it should proceed?

Dr. Grew supposed it to be produced by certain glandules under the skin, after the same manner as the glands in the throat eject continually a slime.

Mr. Hooke supposed it rather to proceed from the transudation of the vapours, or sweat, or rather insensible transpiration of the eels; which vapours coming into the water condense and convert that, which is contiguous, into a slimy substance after the same manner as the seeds of *oculus Christi* put into water condense the water about them into a jelly. And he mentioned, that he had a way of converting slime again into water.

Pores of Skin.

Hereupon it was debated, whether the skin has distinct pores or not. Dr. Grew was of opinion, that it has distinct pores; which he said he could make visible; that they were placed after the form of spherical triangles; and that he could see the sweat issue out of them.

Mr. Hooke supposed the skin to have no distinct pores, but defined or rather described the skin to be a body consisting of two sorts of substances, the one solid, the other fluid: the solid part a close contexture of infinite small fibres every way interwoven like the hairs of wool in a piece of cloth or felt. And this is the reason of the great aptness, which it had for stretching and shrinking any way; as may be also observed in a loosely woven piece of cloth.

The other part is a fluid, which fills the interstices of all these contexted fibres. The fluid is compounded of a more gummous substance and a more watery. The watery part is that, which coming to the outside of the skin is exhaled into the air; by which means the gummous parts are thicker next the air than elsewhere; and if the cuticula be either by fire or other accidental cause thickened too much to hinder the watery part from mixing, and propelling through it, the watery part will gather behind it into a body, and make blisters; or otherwise throw it off in scurf.

Structure of Whale.

July 18. Mr. Hooke gave an account of the structure of the mouth and fins of that whale, which was cast on shore at Greenwich about twenty years before.

He also mentioned a late relation, which he had seen printed in Low Dutch, or a voyage to Spitzbergen or Greenland, wherein was a description of that sort of whale, together with pictures of them.

Oil of Tobacco.

Mr. Hooke remarked, that Sir Christopher Wren had formerly told him, that he knew a maid-servant by the use of oil of tobacco cast into convulsions, which had like to have cost her her life.

Henbane.

Mr. HOOKE mentioned the odd effects, that were wrought upon the children of a poor woman, who used to gather physical herbs for Mr. DRINKWATER, by eating some henbane, which they had mistaken for parsnips: that they all fell stark-mad, but were cured in some short time by the said Mr. DRINKWATER, by the taking of alexipharmics and sweating.

Poisons.

July 25. Upon occasion of a discourse about poisons, Mr. HOOKE mentioned an account, which he had from a Virginia merchant, who had lived long in those parts, and had been very curious and inquisitive into all sorts of natural curiosities; that the only certain cure of the venom of the biting of a rattlesnake was a certain substance found in the wild walnut tree, called by the planters the hilcanes tree, being a kind of spunk, but called by the planters punk. It grows within the body of the tree, and is found by a kind of black hole or navel in the tree; which the planters observing, they presently cut down the tree, and take out the said substance, and preserve it with very great care, being of so sovereign a virtue, besides its other uses for kindling fire, which it catches most readily. The way of using it is thus: as soon as a person finds himself bitten, he immediately takes his flint and steel, and some of this punk, which he always carries about him; and kindles it, and applies the burning punk to the place bitten; and there keeps it burning till he feels the fire (which he will not presently do, because the part bitten will immediately after grow mortified and senseless); and thus he continues it as long as he can endure it: which will certainly cure him without any other after-symptoms; which if the remedy be not presently applied, are very terrible, and often fatal. In confirmation of which last circumstance, the same person had informed Mr. HOOKE, that he knew a man, who had been bitten by one of those rattlesnakes in his finger whilst he was hunting a hare in the woods, having thrust his hand into a hollow tree, where by the baying of his dog he supposed that a hare had sheltered itself. He being bitten, immediately found his hand and arm extremely swollen, with great pain; and then the whole wood seemed to him to turn round; and presently after to be all in a flame: upon which he fell down, and remembered nothing further. Being within a short space of time after found by some of his friends by means of his dog, he was carried home on a ladder senseless, and by means of a surgeon not far distant was so ordered, that he recovered without the loss of his life; but it was three quarters of a year before he was well; and he lost his hair and nails, and his skin peeled off, with many other dreadful symptoms.

Mr. HOOKE mentioned likewise, that he had been informed by Mr. HODGES of Moorfields, that he had known a man, who had cured himself of the pain and swelling of the gout, by applying upon the place quicklime stones, while they were slacking.

Mr. HOOKE, upon this occasion of the growing of the spunk within the body of a tree, said, that it seemed somewhat to resemble the rot in a tooth, which he had observed to have a certain black substance, which covered the surface of the hollowness thereof, which began generally from a small hole in the outside thereof, and so spread itself like a mushroom into the more spongy substance within the outward hard crust.

Punk.

Aug. 1. Mr. HOOKE read the minutes of the last meeting; and then produced a piece of punk given him by the Virginia merchant, who had acquainted him with the former relation and description of it. It was a very light spongy substance, and seemed to be no other than the substance of a fungus or mushroom. The colour of it was brown; and it readily took fire, and would continue to burn till the fire was stifled, but seemed not to burn very fiercely.

Moxa.

Upon this occasion mention was made of the moxa of the East Indies, which cures the gout by the application of it burning to the part affected; and which seemed also to be a kind of spunk.

Springs and electrical Bodies.

Mr. HOOKE read a discourse of his, concerning the nature and power of springs and electrical bodies, giving not only an account of the nature and power of all sorts of springy bodies, and the several phenomena thereof, but likewise of the reasons and grounds of those phenomena; as would more at large appear in the discourse itself, which he designed speedily to publish.[1]

He then exhibited two experiments in order to prove his said theory; the one with a tubical spring of brass wire, and the other with a spiral spring of steel, being the spring of a watch.

Ancient Glass.

Aug. 22. Mr. HOOKE delivered to the society an ancient urn of glass, taken up in Spitalfields upon digging cellars there, presented by Sir CHRISTOPHER WREN for their repository. There was this remarkable in it, that it seemed to be made after quite another manner than that used by the present work-

[1] It was published at London, 1678, in 4to under the title of *Lectures de potentia restitutivâ: or of spring, explaining the nature of springing bodies.*

men in that art, it having no place at the bottom thereof; nor any visible sign how it could be held, whilst the lip and handle thereof were joined to the body.

Experiment with Springs.

Mr. HOOKE showed the experiment of the springing of a string of brass wire, about thirty-six or thirty-seven feet long, extended by weights hung at the lower end thereof; and he made it evident, that the said string extended proportionably to the weight, that was hung to it at the bottom: that is, that one weight extended it one length, two extended it two, and three extended it three spaces or lengths.

[HOOKE's lecture on Springs was printed on Dec. 6 (see p. 503). The figures are reproduced on p. 498.]

'About three years since his Majesty was pleased to see the experiment at White Hall, as also my Spring Watch. Two years since I printed "*Ut tensio si vis*" in *Helioscopes*.'

Animals in Pepper-water.

Mr. HOOKE then showed to the envoy SPANHEIM and his brother, the small animals discovered in pepper-water by the help of a microscope.

Natural History of Spitzbergen.

Aug. 29. Mr. HOOKE produced and read the preface of a book, which he had procured to be translated out of High Dutch, containing a description and natural history of Spitzbergen or Greenland, written by one * * of Hamburg, who had been there himself, and, upon occasion of queries sent out of England, had made it his business to inform himself more particularly concerning all matters therein desired; and by the help of Dr. FOGELIUS of Hamburg, who had translated and delivered those queries to him, had compiled and methodized the same; and for the better illustration of all particulars, had added a great many copper cuts, containing the pictures of the most remarkable particulars, viz. of the whales and other fishes, together with those of the animals, birds, plants, &c. Mr. HOOKE added, that he had delivered the said book to a German, in order to have it translated into English.

Freezing.

Mr. HOOKE related the experiment of producing the regular figures by freezing, by mixing snow and salt in the body of a glass vial, and then putting upon the outside of the same a small quantity of the spirit of urine.

Weather-clock.

Mr. HOOKE was ordered to make what haste he could with

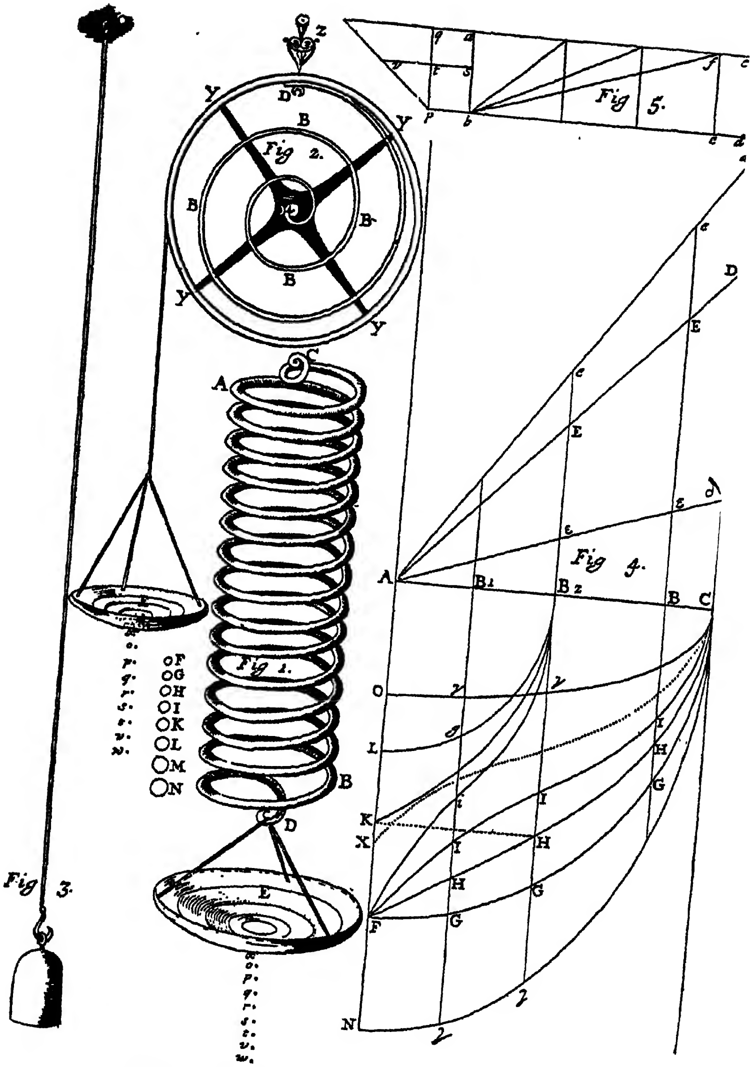


PLATE TO HOOKE'S LECTURE 'OF SPRING' 1678.

FIG. 1. Wire helical spring stretched to points *s*, *p*, *q*, *r*, *s*, *t*, *v*, *w*, by weights F, G, H, I, K, L, M, N.
FIG. 2. Watch spring similarly stretched by weights put in pan.
FIG. 3. The 'Springing of a string of Brass Wire 36 ft. long'.
FIG. 4. Diagram of velocities of springs.
FIG. 5. Diagram of law of ascent and descent of heavy bodies.

the weather-clock, which he designed; as also with a very accurate barometer.

Butterfield on Microscope Lenses, new Levels, Planetarium and sliding half-quadrant.

Oct. 31. Mr. HOOKE read a letter directed to the two secretaries from Mr. MICHAEL BUTTERFIELD, a mathematical instrument maker at Paris, dated there, $\frac{8}{18}$th October, 1678, expressing his desire to correspond with them concerning philosophical, mathematical, and mechanical matters; and offering to communicate such things, as he should meet with there of that kind. He mentioned in that letter Monsieur HUYGENS's method of making small globules of glass for microscopes, by sticking powdered glass on the point of a needle with spittle, and holding them in the flame of a lamp of spirit of wine, and afterwards rubbing them with a putty cloth: as likewise a level of his own invention, published the year before in the *Journal des Scavans*, together with a further improvement of it by hanging it upon one point.

He took notice also of some other sorts of levels invented by other considerable persons: and added, that he was making a large silver planisphere for the King of the invention of Signor CASSINI, of two feet diameter: that he had lately made for Signor CASSINI an equinoctial dial of a foot diameter, with three circles, besides the sliding ring, and two halidades and four sights: that there had been lately made at Paris a piece of clockwork to show the motion of Jupiter's satellites; but that it was not much approved of: and that himself had not long before made a sliding half-quadrant, with a glass of eight inches semi-diameter, of very great use, and easy to be carried.

Volcanic eruption.

Mr. HOOKE read a relation of a great eruption of fire, which happened in the preceding winter in the isle of Palma, one of the Canaries, given him by a person, who had long resided in the said island, and who was there at the time of the said eruption.

Halley's Planisphere.

Nov. 7. Mr. HOOKE showed the planisphere and description of the stars of the southern hemisphere made by Mr. HALLEY. Whereupon Mr. HENSHAW was of opinion, that they would be very acceptable presents to such correspondents abroad, as were lovers of astronomical matters, if the book and planisphere were sent to some of them from the society: to which the society agreed; and it was desired, that the secretaries should send one of these books to Monsieur GALLET, another to Mr. BUTTERFIELD, and a third to the Abbé de la ROCHE, in the name and at the charge of the society.

Mist.

Mr. HOOKE remarked, that it was very common to observe a mist hanging about the tops of hills, when the air above, beneath, and round about was clear.

Earthquakes and Subterraneous Fire.

He also conceived, that there was no earthquake without a subterraneous fire; and that many of those subterraneous fires proceeded from parts very deep in the very body of the earth, and not only near the surface, like the coal mines in the north, which had been set on fire, and had continued so for many years.

Generation of Fire.

Mr. HOOKE mentioned some observations about the generation of fire by rain falling suddenly on a sort of coals, called brass lumps; as also on quicklime lying against deal timber; both which had caused fire. Mr. HENSHAW, Mr. PACKER, and Dr. CROONE instanced in other substances, that would generate an actual fire, such as hay wet, green grass heaped up, malt, cotton-wool, rose-flower leaves, and several other green leaves and plants.

Hair-balls.

Nov. 14. Mr. HOOKE related, that he had often taken the hair of oxen, when they had their coats, and rolling a handful or two of the said hair between his hands, had thereby in a very little time felted them into a very hard and close ball; and that the hairs thereof would naturally range themselves into a most regular and uniform order.

Counterfeit Pearls.

Mr. HENSHAW related the way of making counterfeit pearls used at Bologna and Venice, by silvering or gilding alabaster beads, and dipping them in a size made of fish-glue.

Mr. HOOKE related, that he had done the same thing here, by taking ivory beads instead of alabaster, and proceeding in the manner above mentioned.

Glasses for Microscopes.

He showed his experiments, which were divers ways of making very round and clear globules of glass for microscopes with great ease.

Burning Ditch in Lancashire.

Nov. 21. Mr. HOOKE took notice of the admirable effect, which had been observed in a burning ditch in Lancashire, viz.

that it took fire, though the steam itself were not before sensibly warm; which, he said, was more than any other steam would do; and that therein seemed to be the greatest rarity of it: whence he conjectured, that it might be the same kind of steam, which generates lightning. That this steam seems to be the very same with the fiery damp of mines, which will take fire much after the same manner, but often with more direful effects, by reason that a much greater quantity of the said vapour is collected, and also because it is imprisoned and shut in with very strong sides, and hath no possibility of venting and expending otherwise than by the mouth.

There was much debate concerning the firing of the steam and smoke of bodies, but there was no instance given, wherein steams would take fire, unless they had been before heated to a very great degree of heat by an actual fire, and then kindled.

Insects in Amber.

Nov. 28. Mr. HOOKE affirmed, that he himself had a way of enclosing an insect, such as a fly-worm, or the like, in amber artificially, which could scarce be distinguished from a natural production of that kind.

He added, that he was of opinion, that amber was nothing else but the turpentine or resinous gum of trees, which having lain a long while in the sea or under ground was in process of time petrified, or at least hardened to that degree, in which it was found: which he was the more inclined to believe from the experiment, which Dr. DANIEL COX had tried upon the earth found some years before at Islington; the oil of that earth smelling perfectly like oil of amber, which was made to have that smell from the burying of colophony there by a chemist.

Auditing Accounts and election of Dr. Mayow.

Nov. 30. Mr. HOOKE read the report of the committee appointed to audit the accounts of the Treasurer, Mr. HILL, for the preceding year.

After which the several candidates formerly proposed were balloted for and elected.

2. Dr. MAYOW proposed by Mr. HOOKE, twenty-seven votes, one negative.

Malpighi on Anatomy of Plants.

Dec. 5. Mr. HOOKE presented to the society a discourse, which he had lately received from the President, written by Signor MALPIGHI concerning the anatomy of plants, being a further prosecution of that excellent work of his formerly printed. It was dedicated to the society, and contained, besides a preface

and conclusion, seven several heads or subjects of inquiry. 1. Concerning the vegetation or growth of seeds. 2. Of galls, or the round excrescences growing on an oak. 3. Of the various tumours and excrescences of plants. 4. Of the hairs, down, and thorns of plants. 5. Of the clasps and the like binding parts of plants. 6. Of those plants, which vegetate upon others. 7. Of the roots of plants. Each of these subjects was illustrated by a great number of schemes and delineations most curiously drawn with distinction of black and red for the better explanation. After the reading of the dedication, which testified the author's great respect for the society, it was ordered, that a letter of thanks should be sent by Mr. HOOKE to him; and that Mr. HOOKE should also take care, that the discourse be forthwith printed with all possible correctness; and that a good number of the printed copies be transmitted to the author.

The Precious Stones of Britain.

The minutes of the meeting of November 28, being read, gave occasion of discoursing further concerning the productions of our own country as to rich and precious stones. Mr. HOOKE affirmed it possible to make as good agate-cups as any brought from the Indies, out of certain flints and other stones plentiful enough in England: and that there was no difficulty of doing this, except the charge of the diamond-powder to cut them, which yet might, in some measure, be supplied by emery or other powders.

Dwight's Hard Pottery.

Mr. HOOKE mentioned, that there was a method of making very thick pieces of earth to be burnt, without breaking or chopping: that Mr. DWIGHT had made some heads of earth as big as the life: that his earth was as hard as porphyry: and that the excellency of China earth was, that it would endure the greatest fire without vitrification.

Weather-clock.

Mr. HOOKE produced part of his new weather-clock, which he had been preparing; and which was to keep an account of all the changes of weather, which should happen, viz. 1. The quarters and points, in which the wind should blow. 2. The strength of the wind in that quarter. 3. The heat and cold of the air. 4. The gravity and levity of the air. 5. The dryness and moistness of the air. 6. The quantity of rain, that should fall. 7. The quantity of snow or hail, that shall fall in the winter. 8. The times of the shining of the sun. He was desired to proceed to the finishing of this, which, he said, he hoped to do within a month or six weeks.

Experiment with Condensing Engine.

He promised also to show at the next meeting an experiment in the condensing engine, which he had procured to be now mended and fitted for many experiments.

December 6.

(28) *Lectures, De Potentia Restitutiva,* or, *Of Spring, explaining the power of Springing Bodies: To which are added some Collections.*

See pp. 497–8. *Cutlerian Lectures*, No. 5, 1678.

(29) *Proposals for Printing a new Atlas in the English Tongue in eleven volumes in folio at 40s. a Volume; according to the directions of Sir Christ. Wren; Dr. Vossius, Canon of Windsor; Dr. Pell; Dr. W. Lloyd; Dr. T. Gale and Mr. R. Hooke are to be given away gratis to all Gentlemen that please to send for them to Mr. Pitt bookseller, at the Angel in S. Paul's Churchyard.*

This work was being printed at the Theatre in Oxford on December 6.

Dec. 12. Mr. Hooke produced the condensing engine, which he had caused to be mended and made much more serviceable for trial of several experiments therein, of the effects of condensed air.

An experiment was made to see the effect of condensing the air in a gauge or standard, which was suspended within the cavity thereof.

Experiments on Animals.

It was desired by the society, that it might be tried at the next meeting in the engine upon some animal, in order to see how much longer a creature would continue alive in the engine filled with condensed air than when filled with common air.

It was ordered, that the rarefying engine might be put in order for the trial of the same animals in rarefied air; to see how much sooner an animal will expire in rarefied air than in common air of the same extension.

Breaking of Glass-drops.

Mr. Hooke mentioned, that the manner of the breaking of glass drops might be seen, if the same were dipped two or three times in very clear transparent glue; which, it was desired,

might be prepared accordingly against the next meeting for making the experiment.

Malpighi's Discourse.

Mr. HOOKE reported, that he had treated with Mr. MARTYN, the society's printer, for the well printing Signor MALPIGHI'S discourse lately sent to the society; and that he had desired Mr. MARTYN to print fifty copies extraordinary, to be sent to the author as a present from the society.

Sparks from Steel.

It was further discoursed, that the most conspicuous sparks, that fly from the striking of a flint against a steel, were the small parts of the steel cut off by the flint, and vitrified by the violent motion of the stroke, the sulphur of the iron readily taking fire. For a proof of which, Mr. HOOKE showed an experiment by throwing the filings of iron through the flame of a candle, which immediately kindled and sparkled like gunpowder.

Locality for Gold.

Mr. HOOKE was of opinion, that the original place of gold lay extremely deep in the earth, as being a body heavier than any other yet known, and consequently ought to lie lower than any in order: that had it not been for some former earthquakes and eruptions, it would have still remained in those inaccessible recesses, and so have never been known, as in all probability many other sorts of stones, minerals, ores, and metals, which may lie below the seat of subterraneous fires, may remain concealed and unknown to this day: that by means of subterraneous fires, earthquakes, and other vapours, that cause those effects, those deeper and inner parts of the earth may have been thrown up together with the other effect of the same cause, the very mountain: that that part of it, which is thrown up into the top or body of the mountain, may by the violence of the heat and discharge of the vapours be melted in the raising, and dispersed and broken into a multitude of very small globules, or such like figure, and so be blended and mixed with the earth, sand, &c. of the new eruption: that being thus mixed with the earth or the mountain, the rain falling on the upper part thereof, and descending, washeth down into the rivers those smaller particles of the gold, and so leave them in the bottom of their channels.

Locke on Eclipse of the Moon.

Mr. HOOKE read a letter from Mr. JOHN LOCKE, dated at Padua, giving an account of the late total eclipse of the moon

observed there by an ingenious acquaintance of his, and offering his best endeavours for the service of the society in those parts.

Verdigris and Kermes.

Mr. Hooke was desired to return the society's thanks to Mr. Locke for this letter, and Mr. Henshaw moved, that he might be desired to procure the history of the making of verdigris with wax, and that of the kermes berry, both as to its growth, manner of gathering, preserving, use either for physic, dyeing, or the like.

Origin of Metals.

Dec. 19. There was also a further discourse about the manner of the production of metals in the bowels of mountains. Whereupon Mr. Henshaw declared it as his opinion, that many metals, as gold, were generated and perfected in the superficial parts, as well as in the body of them. But Mr. Hooke endeavoured to explain how those bodies might be original bodies belonging to a much lower region of the earth; and that they were not produced or generated anew, but only by earthquakes and subterraneous eruptions thrown up from their more natural place, and by the violence of those fires melted out of the minerals, in which they were there bedded; and that thence they came to be disposed and scattered into small grains and dust, and found intermixed with very heterogeneous bodies.

Weather-clock and Salary.

Dec. 26. It was ordered, that the treasurer repay Mr. Hooke the five pounds paid by him to Mr. Crawley for workmanship about the weather-clock; as also his salary for half a year ending at Christmas, voted by ballot.

Correspondence and Rooms.

That Mr. Hooke be desired for the future to keep the correspondence of the society; and that the same shall be continued by the help of a small Journal of some particulars read in the society: that the said Journals shall not be sent or sold to any person but members of the society, and to such as correspond with Mr. Hooke by the society's directions, and make considerable returns to him for the society's use; all which returns shall be constantly brought into the society, and read before them at the very next meeting after the receipt thereof. And Mr. Hooke was desired to draw up a specimen of the said Journal propounded by him against the next meeting of the council: voted by ballot.

It was ordered, that Mr. Hooke do agree with Mr. Bradell about two rooms, which he desires of Dr. Pope's lodgings; but that he do not let them under five pounds per annum.

1678/9

Experiments on Respiration.

Jan. 2. Mr. HOOKE gave an account of the experiments [on the use of air for the sustentation of the life of animals] that had been shown [on Dec. 19th], and the design of them, and of the time and manner requisite for the completing such experiments: and he desired, that there might be a committee appointed for the making such trials, as could not be made within the time of the sitting of the society, since many experiments could not be made within so short a space.

Burning of Gunpowder.

Mr. HOOKE showed an experiment with gunpowder, in order to examine whether the burning thereof consumes or produces more air. And upon trial thereof it was found, that[1]

Weather-clock.

Jan. 9. The President, with Sir JOHN LOWTHER, Mr. HENSHAW, Mr. THYNNE, and several others, went up into Mr. HOOKE's turret, to see the further progress, which had been made in the clock contrived by Mr. HOOKE for keeping an account of the several variations of the weather; which was well approved of by them.

Foreign Correspondence.

The minutes of the last meeting of January 2, were read; whereupon mention was made by the President of continuing and promoting correspondence with foreign parts; and it was desired, that the proposal made by Mr. HOOKE should be further considered of by the council, in order to the promoting of that

Gold Dust.

The question concerning the figure of dust or sand-gold was further debated; and it remaining yet a doubt, whether it was found so figured, as had been supposed of regular figures, it was desired, that Mr. HOOKE should endeavour to get a sight of some dust gold, and examine the same with care, to see and describe the most usual figures thereof.

Copper Ore.

On a discourse concerning the mineral of copper, Sir JOHN LOWTHER related, that there was a sort of mineral found in Cumberland, which seemed to be a very rich copper ore; but

[1] This minute was not completed.

that having sent the same to be tried at London, it was found to burn and flame all away like brimstone, and to yield no copper at all.

Mr. HOOKE remarked, that this might probably happen from the mixture and union of some salts and sulphurs with the metal; and that if they were not by some other artifice separated from the metal before the same come to be tried by the violence of the fire, without such artifice or mixture, the sulphur and salts would carry away the metal of the copper, and never suffer it to melt or separate from them into a body. For confirmation of this he alleged, that he knew a way to make even the body of common copper commix again with such heterogeneous bodies, and being put into a fire to fly all away, and burn almost like common brimstone; which copper so ordered might yet be again reduced so as to melt, and not fly away at all.

Respiration.

Mr. HOOKE gave an account of the design of the experiment about respiration, and moved, that there might be a committee appointed of the physicians and such other members of the society, as desired to be further satisfied concerning that inquiry, in order to make that experiment on some other day than that of the meeting of the society, since that experiment could not be tried within the time of meeting: and he proposed it as a very desirable thing for the promoting the ends of the society, that the committee formerly appointed for the trial and examination of several matters might be revived.

Hereupon a committee was appointed to make trial of this experiment of respiration, consisting of Dr. CROONE, Dr. MAPLETOFT, Dr. ALLEN, Mr. HILL, and such others of the society, as should desire to be present at it.

Upon this a discourse arose about respiration.

Dr. CROONE was of opinion, that the steams from the breath and body were the occasion of the death of such animals, as were enclosed in a vessel. Mr. HOOKE objected, that if this were so, then an animal included in condensed air would be stifled by those steams sooner than one included in a vessel, out of which a part of the air was exhausted; whereas the quite contrary appeared by experiment. He added, that he rather conceived it to proceed from the satiating of the dissolving part of the air, and so making the remaining part effete and useless for maintaining the life of animals, which seemed to have much the same nature with flame and fire, since the same effects seemed to happen to it. Hereupon Mr. HENSHAW mentioned the experiment, that had been formerly given to the society; which the President desired be again shown at the next meeting.

Precipitation of Metals from Liquids.

Jan. 16. Mr. HOOKE mentioned an experiment [like one for precipitating gold or silver from a liquid] whereby iron was said to be turned into copper.

Copperas Ore.

Sir JOHN LOWTHER promised to procure some of the copperas ore, in order to its being examined by Mr. HOOKE.

Salt-making.

Upon a discourse concerning the ways of making salt, Mr. HOOKE related, that he had been newly informed by a doctor, who had lived in Ireland, that he had salt-works in the north-west parts of Ireland, where he boiled up the sea-water into salt in iron pans by the help of turf or peat to great profit or advantage.

Combustion in Closed Box.

Mr. HOOKE showed the experiment of putting fire into a tin box, and there keeping it blowed on with bellows, till it appears to have quite lost its burning and shining: then admitting fresh air into the same, and blowing it with the same bellows, it presently rekindled and burned and shined as before. This was thrice repeated to the satisfaction of the members present. But that some further proof of his theory might be examined, he was desired to show some other experiment of this nature at the next meeting.

Flame of a Candle.

Mr. HOOKE showed a second experiment, which was the flame of a candle so placed between the eye and a concave metalline speculum, that the air, which encompassed the said flame, by dissolving the parts of it into itself, became of a different nature and different refraction from that, which was not satiated by the said dissolution.

This was plainly seen by the President and divers others of the members present to their satisfaction.

Spontaneous Generation.

Jan. 23. Hereupon was occasioned a discourse about spontaneous generation. Mr. HOOKE related, that he had been informed by Mr. WYLDE, that he could order earth so, as that without setting or putting any peas at all into it, it should produce peas: as also that the same Mr. WYLDE had told him, that Monsieur le FEBURE, the King's chemist, had assured him, that he having thrown out on a dunghill a pretty large quantity

of the caput mortuum of fennel seeds, from which he had extracted the oil by distillation, had observed the following year, that the whole dunghill was overgrown with young fennel, as if it had been sowed with fennel seeds: concerning which it was conjectured, that the sweepings of the laboratory, which were thrown to the same place, were more probably the cause of this great fruitfulness.

Fresh Water from Salt Water.

There was likewise a discourse about making salt water fresh by filtration; and the opinion was, that no filtration through sand, earth, or the like, would make salt water fresh.

Mr. Hooke observed, that the draining of salt from sea-water was usually very bitter and red, and was for the most part thrown away; only some of it was used for the washing the sores of sheep and cattle, as being a great drier: and he further conceived, that it was made by a dissolution of the iron boiler, and therefore might be of another nature.

Combustion in a Closed Box.

Mr. Hooke showed his experiments; the first of which was the setting a chafing-dish of coals into the box, and suffering it to stay there till it went out, and ceased to shine: then by a hole at the top letting down into the air of the box a wax candle, it would presently be extinguished, as if it were dipped in water; and that as soon as ever the air came to touch the flame of the candle. But that air being changed, and fresh air admitted into the box, the coals began to shine afresh, and the candle let down into the box continued to burn and shine as in the open air.

His second experiment was by putting in a box filled with lighted coals burning clear, when the air had been satiated, as above mentioned, by the coals, which had been set into it: which coals presently ceased to burn, and looked as if they had been quite extinct; which very coals, as soon as ever the fresh air was admitted, presently began to shine and burn as before.

The experiments for the next meeting were to prosecute this theory of Mr. Hooke, that air is a menstruum, that dissolves all sulphureous bodies by burning, and that without air no such dissolution would follow, though the heat applied were as great.

Pressure of the Atmosphere.

Feb. 6. This meeting began with a discourse about the barometer and pressure of the atmosphere, concerning which it was debated, what might be the reason, why the air should press less in rainy than in dry weather. Mr. Hooke supposed, that it might proceed from this, that the air at such time, as it is heavy,

takes up more of the heavy parts of other bodies, and keeps them suspended; whereas in moist and stormy weather the air being of another nature could not be charged with such vapours.

Experiments on Combustion.

Mr. HOOKE showed two experiments in order to illustrate his theory about fire.

The first was the letting down a lighted wax candle into a glass of air, which about ten days before had been satiated by a vessel of live coals put into it, and suffered to remain till they were quite extinct: at which time the vessel was closely stopped up, and had been kept so ever since. The effect of which was, that the candle put into it continued to burn a considerable time, as if it had been fresh air. It was conjectured, that upon the condensation of the air upon cooling after the coals were gone out, the fresh air had made its way into the glass, and so refreshed the air, and made it fit for burning. Others supposed, that the air might have recovered its former nitrous quality by letting fall those parts of the coals, which it had formerly dissolved. It was desired, that this should be further and more carefully prosecuted.

The second experiment was to show, that a coal, though kept in a very great heat, would not be consumed or burnt, unless there were an access of fresh air. There was a charcoal included in a cylinder of glass, and so perfectly sealed up hermetically: then this glass was put into a chafing-dish of live coals, and suffered to lie there a considerable time in a heat great enough to melt the glass, so that the glass shaped itself into the form of the coal: notwithstanding which the coal remained unconsumed, and manifested the necessity of air for making an actual fire.

The experiments for the next meeting were to be such, as should further demonstrate the former theory.

Luminescence of Burning Charcoal.

Feb. 13. Mr. HOOKE showed an experiment, which he had mentioned at the last meeting; the design of which was to see, whether a coal heated to a degree sufficient to melt the glass, that included it, would by that violence of heat be consumed or made to shine and give light. To this end a piece of charcoal was included in a urinal glass so ordered by the means of wires, that the charcoal remained in the middle of the belly of the urinal without touching either the sides or bottom. Then this urinal was placed upon a chafing-dish of clear burning coals, and so suffered to stand till the belly of the glass was red-hot; at which time the mouth of the urinal was close stopped with clay. Then the coals in the chafing-dish were blowed upon very quick

with bellows, whereby the lower parts of the urinal melted, and were thrust inwards by the coals, on which it stood: notwithstanding which great heat the coal in the middle of the glass did not burn or shine, or at all seem to consume, till by taking off the clay, that stopped the mouth, and blowing in fresh air with the bellows into the body of the glass, the shining, burning, and dissolution of the coal plainly appeared: and that coal, which before in a much greater heat had remained black, as if cold, now in a much less heat grew all over red, lighted, and presently after the white ashes covered its surface: which was judged an evident sign, that the heat of a fire, though exceedingly hot, was not able to burn a combustible body without air; and that the air was the body, that wrought the effect upon the combustible body.

Feb. 18.

LECTIONES CUTLERIANAE, or A Collection of Lectures, Physical, Mechanical, Geographical and Astronomical, made before the Royal Society at Gresham College. To which are added Divers Miscellaneous Discourses.

Theory of Barometer.

Feb. 20. The minutes of the last meeting of the 13th being read gave occasion to discourse further concerning the theory of the barometer, from what causes the alteration thereof might proceed. Some were of opinion, that the cause thereof might proceed from the extraordinary height of the air only ebbing and flowing as it were like a tide, but with unconstant motions.

Mr. HOOKE was of opinion, that to this was to be joined the particular and specific gravity of the body of the air, as being charged sometimes with heavier, sometimes with lighter vapours or bodies dissolved into it, or taken up by it. He further explained how different heights might produce the same pressure, provided there were the same quantity of gravitating within the same cylinder: and he instanced, that a cylinder of the same air rarefied into greater height in summer could have no more pressure, than when in the winter time it is condensed into a much shorter. Further to elucidate his theory of it he added, that he would at the next meeting produce some experiments.

It was desired, that the experiments formerly propounded by Mr. HOOKE to be tried at the column on Fish Street Hill might be anew prepared: and in order thereunto Mr. HOOKE was desired to provide convenient glasses and other conveniences

for the perfecting that trial: and that then the trial might be made again with all the care and exactness necessary.

Mr. HENSHAW desired, that Mr. HOOKE would again show this experiment, which he had formerly produced, in order to explain his hypothesis of the reason of the alteration of the pressure of the atmosphere.

Theory of Fire.

Mr. HOOKE produced two experiments in order to make his theory of fire more evident.

The first was a charcoal weighing a hundred and twenty-eight grains put into a box of iron with sand enough to fill the cavity of the box not filled by the charcoal, and therein screwed up very close by an iron screw-pin. Then it was put into the fire, and there for the space of two hours kept very hot, viz. of a bright red-hot. After which the iron was taken out of the fire and suffered to cool; then opened; and the coal being taken out and weighed, was found to have lost but a grain and half of its weight; which was attributed to the moisture, that might be in the said coal when put in. It was further remarkable, that the shape of the outside of the coal was not altered, nor anyway consumed.

The second experiment was the setting of a crucible full of nitre in a very hot fire; in which it was made red-hot, and the petre was found not to burn till a sulphureous substance was put into it, such as wood, coal, brimstone, or the like, upon the injecting of any of which there were presently produced a fire and flame, by which those substances were consumed. Whence Mr. HOOKE argued, that the nitrous part of the petre was that, which corroded the sulphureous body, and thereby the alkalizate salt of the petre was left behind, and augmented by parts of the coal taken into itself.

Librarian.

Feb. 27. It was ordered, that Mr. HOOKE have time till the Thursday following to consider proposals [for the office of Library Keeper], in order to return his final answer: And, that upon his refusal Mr. PERRY have the offer of keeping the library.

Preparation of Saltpetre.

Mr. HOOKE mentioned the way of making saltpetre with spirit of nitre and alkali salt mixed, whereby it appeared, that saltpetre might be compounded of an alkalizate and an acid salt mixed together, and so coalescing into saltpetre.

Halley's Magnetical Observations.

Mr. HALLEY related two magnetical observations, which he

had made in his voyage to Saint Helena: the first was, that the dipping-needle lay horizontal at about fifteen degrees on this side of the equinoctial line: the second was, that northward of that place an iron being held perpendicular, the lower end would attract the south pole, and southward of it the lower end attracted the north.

The reason of which Mr. HOOKE supposed to be, because the northern magnetical pole (as he had formerly showed at the society from the examination of the variations observed in Captain JAMES's voyage to Hudson Bay) was placed beyond the pole of the earth towards our horizon, and not between the pole of the earth and us, as Mr. BOND supposed; and because the motion thereof was from west to east contrary to what Mr. BOND supposed; which was, that it moved from east to west: and because that the said pole was within the body of the earth, and not in the air, as Mr. BOND supposed.

Pressure of Vapours.

The experiment showed by Mr. HOOKE was to show, that vapours press only according to their own gravity, and not according to the space, which they take up in the atmosphere. This was done by a bladder blown up under water: but the consequences of it not being so evident to some of the members present, it was desired, that some more convenient glasses should be prepared for it against the next meeting.

Amanuensis.

Mar. 6. It was ordered, that Mr. HOOKE shall employ such person as he has proposed for the writing letters, as he shall think fit.

Saltpetre.

Mr. HOOKE conceived, that nitre from its manner of production might be supposed to consist of two kinds of salts united together into one *compositum*; the one a very volatile and aerial salt rarefied and flying in the air; the other an earthy fixed and alkalizate salt mixed with the earth, by the union of which with one another they become strictly joined into one body, which composes a vitriolate salt.

Objections were made against the supposition, that the sea receives its saltness from the parts of the earth contiguous to it: and it was said, that there was known but one lake in the world, that yielded salt water, though it is most probable, that, were that the cause, there would be many more found to contain salt water.

Gunpowder.

Mar. 20. The minutes of March 13, were read, which gave occasion to discourse concerning the ingredients of gunpowder.

Mr. HOOKE conceived, that one great cause of the sudden expansion of the powder was the operation of the alkalizate salt in the charcoal, that served to compound the powder.

Bacon on Old Age.

Dr. GALE promised to lend his copy of BACON's book of the prolongation of life to Mr. HOOKE to peruse, when he should call for it.

Theory of Fire.

After this the discourse was about fire, flame, heat, &c. and several objections were made against Mr. HOOKE's hypothesis of explaining it by the dissolution of bodies by the air. It was supposed, that the fire went out when all the pores of the air were filled so, as there was no more space left for the vapours and smoke to fly out of the coal. To which he answered, that the want of room could not be the cause of the extinction of fire, because if the air were drawn out of the vessel, wherein a candle or coal were burning, it would yet sooner go out; whereas there was thereby manifestly made more room; and upon the forcing and compressing more air into a vessel, the fire would continue so much the longer burning.

1679

The Inventions of Roger Bacon.

Mar. 27. Dr. GALE moved, that Mr. HOOKE might peruse the epistle of ROGER BACON to Pope CLEMENT, and take notice of what was considerable about any invention supposed to have been the product of a much later age; since in that epistle, as Dr. GALE conceived, is the epitome of all his inventions mentioned elsewhere in his works: and that therefore Mr. HOOKE might soon see what might be expected.

Dr. PLOT was desired to collate it with the Oxford manuscripts.

Combustion under Water.

Mr. HILL proposed, that it might be tried, whether anything either natural or artificial would burn in or under water. To which Mr. HOOKE answered, that this effect might be performed with gunpowder.

Roger Bacon's De Motu Naturali Mirabili.

Apr. 3. Mr. HOOKE was desired to consider of that paper

[ROGER BACON, *De Motu Naturali Mirabili*] to see if he could explain the meaning thereof.

Sparks.

Mr. HOOKE mentioned his observation of the melting of a small particle of steel struck off by the violence of the stroke of a flint; the heating of iron by filing, hammering, grinding, rubbing: that the dust thrown off from the grindstone in grinding knives, razors, and the like, are melted globules of the steel: that the particles of flints and other hard stones upon striking one against the other will grow red-hot and shine; as will also a tobacco-pipe clay, and several other clays and earths hardened by burning.

Combustion under Water.

Mr. HOOKE related, that he had fired a gunpowder serpent, which had burnt under water, and came out again burning after it had passed through a good space of water.

Ignition of Varnish.

Sir JOHN HOSKYNS related, that the steam of the varnish used by cabinet-makers would sometimes take fire, and had been observed to run along till it set fire to the varnish in the vessel.

Mr. HOOKE related, that he had observed something of this nature of oil of turpentine; which being mixed with an acid liquor, set it into a most violent fermenting, heating, and smoking, insomuch that it could not be endured in the room: being set in the chimney corner, where a fire was, the smoke immediately caught fire, and burned with a blaze almost a yard high.

To measure Rain.

Mr. HOOKE showed an experiment in mechanics; which was a way how to take notice of all the rain, that falls, and was designed as a part of the weather-clock. The contrivance of this invention was the suspending the bucket, that was to receive the quantity of rain, that falls at any time (whether more or less) so that according to the quantity therein contained, the place thereof should either be higher or lower; but certainly determined. This was performed by a counterpoise to the said bucket. The counterpoise was contrived two ways, either by a string of leaden bullets so ordered, that when the bucket was quite empty, all the bullets rested upon a table; but when there fell as much water into the bucket, as equalled the weight of one of the leaden beads, then the bucket descended one space, and one bullet was lifted up; and when twice as much, two bullets, and when three times as much, three bullets were lifted, and so forward, till all the bullets were lifted up, and the bucket had

descended to its place of emptying; whereupon the chain of bullets presently descended and lifted up the bucket into its empty place. But because this motion proceeded with jumps, and was not continually equal, therefore a second contrivance was also shown; which was this:

The counterpoise to the bucket, when empty, was a cylinder immersed into water, mercury or any other fluid; which cylindrical counterpoise, according as the bucket received more and more water, was continually lifted higher and higher out of the water by spaces, always proportioned to the quantity of water, that was contained in the bucket. And when the bucket was filled to its designed fullness, it immediately emptied itself of the water, and the cylinder plunged itself into the water, and raised the bucket to the place, where it was again to begin its descent.

This contrivance here made use of was declared to be useful for making a new and useful beam for examining the weight of bodies without any trouble of adjusting, the rising of the cylinder immediately showing the determinate weight of any body put into the scale without any further trouble.

[This matter is further discussed on p. 521.]

Knowledge of the Ancients before Roger Bacon.

Apr. 10. Mr. Hooke remarked, that though the [ancients] had not gunpowder, yet that by the help of great springs they were able to do very great things: that besides divers other springy bodies, they knew the use of the spring of the air, as is evident in Hero's works, especially in that mentioned by Vitruvius for the quenching of fire.

Hygroscope made with Gut.

Mr. Hooke showed his experiment, being an hygroscope made of several short gut strings, or any other shrinking body sensible of the moisture and dryness of the air. These were united together by the means of iron wires made after the manner of beams, that the shrinking and swelling of every one of them was communicated to the last, which moved the index, by which means the least mutations of the air, as to dryness and moisture, were made very sensible, and the contrivance of it was so ordered, that the least degree of power, which it had to stretch or shrink the string, would easily move and make a sensible alteration.

Some objected, that gut strings would in process of time lose the power of shrinking and stretching, and so were unfit for such a work. But it was answered, that those strings were not intended for a constant hygroscope; but only for the present use,

to show the manner how to make use of these, or any other shrinking or swelling body for the showing of the effect desired: and that at the next meeting there would be one of another substance produced, that would not be liable to those objections.

Hygroscope made with Elm Board.

Apr. 17. Mr. HOOKE showed his experiment, which was a way of making an hygroscope with pieces of elm cut across the grain, the better to be able to be sensible of the changes of the air, so contrived as to make the same as much and large as should be desired. That produced by him consisted of twenty-four feet of the said pieces, and might be made of a hundred or two hundred feet of the said pieces, and yet so ordered, that the shrinking and swelling of every foot of it was made sensible, and accumulated all in the last. He was desired to complete this invention for the weather-clock, which was now near finished.

April 24. PETER NELSON wrote concerning Mr. SMITH's Table of Equations. [MS. *Sloane* 1039, f. 103.]

May 1. Mr. HOOKE produced his new way of ordering pieces of elm for the making of an hygroscope: upon which it was desired, that there should be one of this kind prepared to stand in the meeting-room of the society: as also that a barometer, after Mr. HOOKE's way, should be prepared for the said room.

Flying Machines.

May 8. Mr. HOOKE produced and read a paper, containing a description of the way of flying, invented and practised by one Monsieur BESNIER, a smith of Sable in the county of Maine-et-Loire, the contrivance of which consisted in ordering four wings folding and shutting like folding [ ], to be moved by his hands before and legs behind, so as to move diagonally, and to counterpoise each other: by which he was, it was said, able to fly from a high place cross a river to a pretty distance.

Mr. HOOKE produced a model of the contrivance of the wings made with pasteboard, whereby both the manner of the motion of them diagonally, and also of their opening and shutting, was explained; though he supposed that not to be the best way contrived for the performing that effect after that manner, but that the same sort of wings might be much more advantageously made and used for that effect.

Phosphorus.

May 15. Mr. HOOKE gave an account of his trials made with the phosphorus of Mr. SLARE, that having exposed it for several minutes to the rays of the moon at full and near the meridian,

he could not perceive the least appearance of light, though carried into a very dark room; and that he was not able to find any effect of the light of the moon, though cast upon the phosphorus by a large reflecting burning glass: which agreed with the observations made of the same by Mr. HENSHAW.

Experiment on Sun Light.

The experiment designed to be exhibited by Mr. HOOKE being an experiment of light to be shown by the rays of the sun, could not be performed by reason that the afternoon proved cloudy.

Weather-clock.

May 22. A stranger being present, the weather-clock now finished by Mr. HOOKE was omitted to be shown till the next meeting, when it was to be carried to Mr. HUNT's lodging, that he might attend it and supply it with fresh papers.

M. Papin's new Experiment.

Mr. HOOKE then had leave to introduce Monsieur PAPIN, a gentleman, who stayed in the outer room with an intention to show an experiment to the society, which was singular and new.

Lana's Flying Machine.

May 29. Mr. HOOKE read a translation of a chapter of the Italian book of Father FRANCISCO LANA, entitled *Prodromo*, being an explication and demonstration, as he supposed, of a way to make a vessel to swim and float in the air, so as to carry in it one or more men with other heavy bodies, invented, as he says, by himself, in order to make flying practicable, which had before been thought impossible.[1]

Grain Gold.

Mr. HILL produced from Mr. THOMAS CRISPE a parcel of grain gold, which Mr. HOOKE having examined with a microscope, found to consist of small bulks of very irregular figures; but that most of them seemed to have been melted, all the angles of them being round and swelling, and not at all like the angles of sand, which are sharp; and it was conceived, that the same kind of figures would be produced, if the gold, when melted, were dashed into a heap of sand.

Weather-clock.

The society then went to take a view of the new weather-clock, which was set up in Mr. HUNT's lodgings, made to keep an account of the quantity and time of all the changes, that

[1] See Mr. HOOKE's *Philosophical Collections*, No. 1, p. 18.

happen in the air, as to its heat and cold, its dryness and moisture, its gravity and levity; as also of the time and quantity of the rain, snow, and hail, that fall: all which it sets down in a paper, so as to be very legible and certain.

About *Dec.* 5, 1678.
Dr. HOOK's *description of his Weather-wiser.* [Derham, p. 41.]

The weather-clock consists of two parts; *First*, that which measures the time, which is a strong and large pendulum-clock, which moves a week, with once winding up, and is sufficient to turn a cylinder (upon which the paper is rolled) twice round in a day, and also to lift a hammer for striking the punches, once every quarter of an hour.

Secondly, of several instruments for measuring the degrees of alteration, in the several things, to be observed. The first is, the barometer, which moves the first punch, an inch and half, serving to shew the difference between the greatest and the least pressure of the air. The second is, the thermometer, which moves the punch that shews the differences between the greatest heat in summer, and the least in winter. The third is, the hygroscope, moving the punch, which shews the difference between the moistest and driest airs. The fourth is, the rain-bucket, serving to shew the quantity of rain that falls; this hath two parts or punches; the first, to shew what part of the bucket is fill'd, when there falls not enough to make it empty itself; the second, to shew how many full buckets have been emptied. The fifth is, the wind vane; this hath also two parts; the first to shew the strength of the wind, which is observed by the number of revolutions in the vane-mill, and marked by three punches; the first marks every 10,000 revolutions, the second every 1,000, and the third every 100: the second, to shew the quarters of the wind, this hath four punches; the first with one point, marking the North quarters, *viz.* N.: N. by E.: N. by W.: NNE.: NNW.: NE. by N. and NW. by N.: NE. and NW. The second hath two points, marking the East and its quarters. The third hath three points, marking the South and its quarters. The fourth hath four points, marking the West and its quarters. Some of these punches give one mark, every 100 revolutions of the vane-mill.

The stations or places of the first four punches are marked on

a scrowl of paper, by the clock-hammer, falling every quarter of an hour. The punches, belonging to the fifth, are marked on the said scrowl, by the revolutions of the vane, which are accounted by a small numerator, standing at the top of the clock-case, which is moved by the vane-mill.

Dr. HOOK'S *contrivance of a vessel, to measure the quantities of rain falling: being a part of his Weather-wiser in the preceding paper.* [Derham, p. 43.]

PROBLEM.

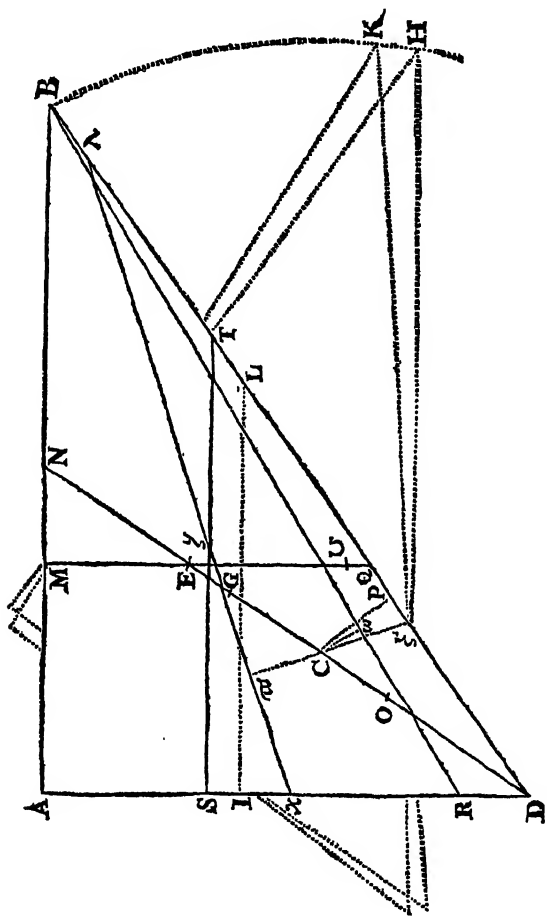
To make a vessel, which, when it hath received a certain quantity of water, shall empty itself.

Let the vessel be a triangular prism, as *z* poiz'd like a balance upon a foot, so that the lesser end may also descend, and not the greater, by means of the stop D. And let one of the sides be ABD. From N, the half of AB, draw the line DN; and from M⅓ of AB, draw MQ parallel to AB; therefore E shall be the center of gravity of the triangle ABD. And because AB is an open side of the vessel, some point between E and D, as G, shall be the center of gravity of the whole vessel; taking a point at P near Q, towards D, erect PC, and let C be one of the centers of motion, upon which, and the like opposite point in the other side of the vessel, it shall turn as a balance. *Secondly*, by adding weight in O opposite to G, equiponderate the whole vessel upon the center of motion C; therefore DCN will be a balance, whose center is C, and the weights of equal moment are G and O. *Thirdly*, draw the line ST parallel to AB, so that C may be the center of gravity of the triangle DST.

First, I say, if the vessel be fill'd short of ST, the side D shall preponderate; if higher, the side B; because C is the center of the balance DCN, and the centers of gravity of all the like triangles, less then DST (as DIL) are upon the arm DC, and the centers of all the greater upon the arm CN. Hence it follows, that because it is stopp'd from descending at D, the vessel shall rest till the water rise above ST, when the side, towards B, shall preponderate.

2dly, I say, if the vessel be inclined towards B, the part B shall still preponderate; let ABD be inclined, (C the center as before;) so that the water, that lay before at ST, lies now as $\kappa\zeta\lambda$,

and let $\pi C \rho$ be a perpendicular line, because the triangles DST, $D\kappa\lambda$ are equal, but $\zeta\kappa S$, the triangle nearer the perpendicular, is taken away, and $\zeta\lambda T$, being farther off, is added on the side



towards B; therefore that side preponderates, and the more the lower it descends, because the center of gravity, of the triangle $\zeta\lambda T$, runs farther and farther from the perpendicular, till it runs over at B.

3*dly*, I say, that when a part given of the water is poured out, the residue still preponderates, while it remains inclin'd. Let

the water be represented by the triangle DRB in the motion of pouring out, part being run over; the center of gravity of the water, is v in the line MQ: and $C\omega$ at right angles to BR, will be the perpendicular, as CP will be the perpendicular when B is descended so low, that DB becomes horizontal, (that is, when all the water must be poured out) therefore CP is between CO and v, but by construction the nearest point of MQ is without CP towards B, therefore v preponderates; therefore the vessel still inclines, till all be poured out. Therefore that, which was required, is perform'd.

Scholium.

If it be requir'd that the vessel, after it is empty, should return again to its former position, there must be added to the point O yet more weight at K, enough to restore the emptied vessel, in which case a triangle may be drawn as DBR, whose weight upon its center v shall equiponderate to K in O; it seems therefore, that the vessel should descend no lower than till BR be horizontal. But because nothing that moves towards an equilibrium rests there, but is carried further by the impress'd force which it gains in descending to this equilibrium, as it appears in all manner of pendulous motions. And because K may be less than any magnitude assigned, therefore, notwithstanding the counterpoise of K, it will descend so low, as to pour out all; that is, having gain'd an impress'd force in its descent from B to K, there is no reason but it should continue it beyond the equilibrium to H and further.

June 1679.

There is lately set forth a Proposal towards the finishing of two large Hemispheres *in Plano* of above 30 inches diameter, projected upon the poles of the world; containing all the northern and southern Constellations. . . . The corners about the Hemispheres are supplyed with divers Systems of the World, Hevelius's Faces of the Moon and other ornaments.

Approved by us Jonas Moore Knight, F. Bar-

nard, R. Hooke, Edm. Halley, J. Leek, J. Collins, Wm. Leybourn.

Utility of Papin's method of softening Bones.

June 5. Mr. HOOKE proposed [Monsieur PAPIN'S way of softening bones] as a very useful mechanical operation for making of inlaid works with bones or ivory, to stain them with colours while soft, to see, if they would hold those colours, when dry and hardened again: and he affirmed, that he knew a method of dyeing ivory as black as jet, and sinking the colour into a considerable depth, so as to be very lasting, without softening the ivory; but that he could not do the same thing with other colours.

Lana's Flying Attempts.

Mr. HOOKE read a further discourse of Padre LANA concerning flying, which he had translated; and added to it a discourse of the impossibility of that attempt by that means; and also showed wherein the author had been greatly mistaken in the grounds and suppositions of his demonstration, viz. in supposing the same thickness of metal to be sufficient to resist the pressure of the air inward in a ball of twenty-four feet diameter as in a ball of one foot diameter: whereas on the contrary it is necessary to increase the weight of the shell more than according to the proportion of the solidity or capacity of the ball.

Microscopic Appearance of Vine Tendril.

June 12. The fourth was the tendril of a vine brought by Mr. THOMAS CRISPE; which being examined by Mr. HOOKE with a microscope was found to have a good number of small plants seeming a kind of moss growing on it; the stalks whereof were about half an inch long, and as fine as the hair of a man's head; at the end of each of which grew a pod much like that of seeding moss, but very much smaller.

Cocoa-nut Milk.

June 26. Mr. HOOKE produced an entire cocoa-nut, which was newly brought from Barbados; and he caused it to be cut in sunder, and poured out of the middle of it a glass full of liquor containing about a third part of a pint. This liquor was something whitish, and tasted sweetish and pleasant like an emulsion. It was contained in the cavity of the kernel, which might be capacious enough to hold about a pint. The kernel was about three-fourths of an inch thick, lining the inside of the shell, which was about one-eighth of an inch thick and very hard. The kernel was much of the same taste with the liquor, but pretty hard and tough.

Impenetrable Armour.

Mr. HOOKE produced the breast part of a new sort of armour made of silk well quilted together, which was able to resist the shot of a pistol or carbine. It was near three-fourths of an inch thick, and very hard and pretty heavy. Whether it would perform what was pretended concerning it, the society was not convinced by any experiment: only the gentleman, who owned it, showed it with two bullets, which had been shot against it by the Prince in the King's presence, which were very much battered and flatted against it without at all penetrating it.

Papin engaged as Amanuensis.

July 3. It was ordered, that Mr. HOOKE shall have power to employ Monsieur PAPIN for the writing of all such letters, as shall be ordered, to the correspondents of the society: and that all such letters shall be transcribed into a Letter Book to be kept by the Secretary of the society: and that all the said letters, when fairly written, shall be shown to such person of the society, as the council shall appoint from time to time for viewing of them before they be sent to the correspondents, and after such perusal shall be sent by the Secretary as directed: and that for so doing the said Monsieur PAPIN shall receive from the Treasurer of the society the sum of eighteen pence per letter, unless the letter shall exceed two sides of a quarter of a sheet of paper; for every of which he shall receive two shillings, he producing his bill of the number of such letters signed and attested by the Secretary:

That the letters being approved of by any two of the council shall be sealed and sent as above.

Publication of Philosophical Matters.

That Mr. HOOKE be desired to publish (as he hath now declared he is ready to do) a sheet or two every fortnight of such philosophical matters, as he shall meet with from his correspondents; not making use of anything contained in the Register Books of the society without the leave of the council and author.

Cocoa-nut Milk.

Mr. HOOKE was desired to examine PISO's *Natural History*, to see, if any light touching Grew's conjecture [that the milk would thicken into a kernel] might be obtained.

Papin's Boiler.

Monsieur PAPIN was introduced by Mr. HOOKE to show the society some further trials, which he had made of his invention of boiling, upon tortoise-shell and ivory.

Swammerdam's Catalogues.

Mr. HOOKE produced a packet of books, which were sent to him directed for the society; which being opened were found to be some catalogues of Dr. SWAMMERDAM's rarities, one of which was reserved for the library, and the rest distributed to several of the members.

Saline Waters.

There was some discourse held concerning waters, whether those, that were impregnated with alkalizate salts, would draw tincture more plentifully than fresh water. Dr. GREW was of opinion, that they would: but Mr. HOOKE conceived, that fresh water would draw a much stronger tincture from a plant before it was impregnated with salt than after; for which he alleged many reasons, and instanced particularly in the way of drawing tinctures from senna, the one with fair water, the other with water impregnated with salt of tartar; the first of which would be the strongest, though the other appeared deeper.

Spermatozoa of a Horse.

Dr. SLARE was introduced by Mr. HOOKE to show the society the experiments of animals *in semine animalium*. He brought with him the liquor, which he had lately expressed out of the testicle of a stone-horse, which had been newly gelt. This liquor he took up in small canes, and viewed them with a single microscope, whereby they were made visible: but Mr. HOOKE putting some of the liquor upon the plate of his double microscope, an infinite number of those small wriggling creatures might very plainly be distinguished, and were discovered and observed by most of the members, who were present.

Cocoa-nut Milk.

July 10. The minutes of July 3 were read, and Mr. HOOKE was desired to examine PISO about the cocoa-nut.

Tortoise-shell softened and hardened.

Monsieur PAPIN produced a piece of the tortoise-shell, which he had shown at the last meeting soft like wet tanned leather, now reduced to its former hardness and transparency.

Tanning Leather.

Upon this several discourses arose about tanned leather; and Mr. HOOKE urged, that the tanning of leather consisted only in the drawing out of the glutinous part of the skin, and the leaving the fibrous part; which was the reason why skins not well tanned being wet and dried again become stiff and hard, the remaining gummous parts glueing the fibres together; whereas tanned

leather being wet and dried again becomes limber and supple as cloth, it having no gummous parts left. Whereupon divers discourses were occasioned about the several ways of tanning leather.

Leewenhoeck's Letter on Seed of Animals.

Mr. HOOKE read a translation, which he had made of a letter of Mr. LEEWENHOECK formerly read by Dr. GREW, in which the writer gave an account of his observations made on the seed of animals, as of fishes, birds, and beasts, in all which, he affirms, that he had discovered with his microscopes vast quantities of living creatures exceedingly small: to which he annexed a paper, in which he had calculated both the number of these animals in the milt of a cod-fish, and the number of men at one time upon the habitable face of the earth, and concludes, that the number of the former exceeds the latter at least ten times.

The Monument.

July 10. On this day HOOKE signed a report after surveying the Monument.[1]

Lorenzini's Observations.

July 17. Two copies of STEPHANO LORENZINI'S *Observationi interno alle Torpedini*, printed at Florence in 1678 were recommended to Sir JOHN HOSKYNS and Mr. HOOKE to be perused by them, who were desired to give some account of the contents of that book at the next meeting.

Cocoa-nut Milk.

Mr. HOOKE gave an account of his having perused PISO about the cocoa-nut, and related, that this author affirms the young cocoa-nut to be full of a pleasant milky juice, but that when it is fully ripe, that juice is dried away, and there is only a hollow kernel left within the hard cocoa-shell. Upon this occasion mention being made of the Indians climbing those palm-trees, Mr. HOOKE described the way of their doing it by the use of two short ropes, the one fastened to their feet, the other to their arms, by the help of which they could climb to the top of the straightest, smoothest, and highest trees.

Renewing of Wines.

Sir THEODORE DE VAUX mentioned a way of renewing and

[1] 'The Monument on Fish Street Hill, built by Wren, and completed in 1677, had been used by Hooke and other members of the Royal Society for Astronomical purposes, but abandoned on account of the vibrations being too great for the nicety required in their observations.' Elmes' *Life of Wren*, p. 289. It is curious that this gave rise to the report that it was unsafe.

An account of the quantity and Value of the work done at the Column on fishstreet Hill.

The solidity of the whole fabrick from the Bottom of the lowest plinth to the Black marble under the urne, the cylinder of the staircase only deducted, ~~[illegible]~~ and the stone for the carving not allowed for, is - - - - 37396

The Black marble ye covers ye capitall - - 287

Lanthorn — 64.

From this solidity Deduct

For 8 great neches - 281
For 3 doors & passages - 289
For 3 sides Renegled - 486
For Rough Block - 1499
For Rubble work - 7185

in all - 9740

The Remainder is 27656

To this adde upon the account of the carving in the front, Dragons & festoons - - - - 540

28196

For marble stepps.

For marble paving.

For marble Harth pace.

This is the true account of the measurements taken by Mr. Laybourne by my direction and assistance and the computation made thereupon by us both. all wch was done together wth the assistance and in the presence of Mrs Marshalls frends & servants by us — witnesse my hand — Robert Hooke

July the 10th 1679 —

There is demanded 39 foot of solid stone at the top of the Column more then what I could measure.

HOOKE'S AUTOGRAPH SURVEY OF THE MONUMENT.

refreshing palled and decayed wines by putting raisins in it. And Mr. HOOKE said, that he had known a merchant, who made use of the juice of English grapes to renew his wines.

Lamb's Testicle.

Mr. HOOKE showed two experiments: the first was the testicle of a lamb, which being dissolved, and the liquor contained in it examined in a microscope, it was found not to have any live animals, but to be exceedingly full of the small globules. Whether there had been any creatures in it, and were now dead, by reason that the lamb had been killed in the morning; or whether there were not as yet any living creatures in it, the lamb being not come to maturity for generation, could not be distinguished. But further trial in order to this inquiry was desired to be made on a young lamb's stone, as soon as the creature should be killed.

Papin's Exhausting Engine.

The second experiment of Mr. HOOKE was with the exhausting engine of Monsieur PAPIN: and that was with a long helical spring of brass wire extended by a weight hung at the lower end of it, the upper end of the same being fastened to the top of a long glass cone. Out of this cone the air was well exhausted, and the station and length of the spring was curiously observed: then the air was let in, and the same observations were made with the same cone; and it was found, that the whole pressure of the air did not in the least alter the stiffness of the spring; which cleared that dispute, whether the unequal motion of a watch does not proceed from the alteration made on the spring by the various pressure of the air thereupon.

New Book on 'Optic Glasses'.

July 24. Mr. HAAK produced a book entitled *Propositions of Optic Glasses*, printed at the theatre at Oxford.

Mr. HOOKE, who had read somewhat of the book said, that he had not found anything in it, which was new; and that it contained some propositions about the place of the image, which were not true: that it came far short of the theory of optics now well known, which he conceived to have been first well understood by KEPLER, and highly improved by DESCARTES.

Overflowing of a River in Gascony.

Mr. HOOKE read a long paper, which he had translated from the French, giving an account of the prodigious overflowing of a river in Gascoigne, written by a person, who had made it his business to inquire into the truth of the fact, and had likewise been inquisitive after the cause, which he explained and illus-

trated by divers very convincing circumstances, ascribing it to the * * * * of some of the Pyrenean mountains into the subterraneous cisterns within the bowels of them.[1]

This occasioned a discourse of the effects of the like nature; and Sir JONAS MOORE and Mr. HOOKE mentioned some, that had happened at Pendel Hill, of which an account had been given in a letter read to the society.

Lorenzini on the Torpedo.

Mr. HOOKE was desired to get the Italian book translated into English, and printed; which he promised to endeavour to do.

Tract on Optics.

Mr. HOOKE, who had some years since seen that tract of in Mr. COLLINS's hands, judging it to be very good and fit for publication.

Glasses for Short-sight.

July 31. Mr. HOOKE read a discourse of his concerning the use, which he had found of convex glasses for helping short-sighted people to see objects at any distance very distinct and bigger than any one, which the naked eye can distinguish; which is a discovery of much benefit to short-sighted persons.[2] He called his discovery *myopibus juvamen*, and observed, that by the use of seeing things inverted it became as natural as if they were seen erect; and he conceived, that if a person from his childhood were used to see things by this means inverted as we call them, though they were really erect in the eye, if they should afterwards come to see them without the help of these glasses, they would conceive, that they saw them inverted, as they really are, at the bottom of the eye, as he said was very visible in a young cat's eye, which is almost transparent at the bottom.

Parallax.

Mr. COLWALL gave an account from Mr. FLAMSTEAD, that he had prosecuted the observation begun and invented by Mr. HOOKE of observing the parallax of the earth's orb among the first stars by a perpendicular telescope; and that he had certainly found what it is. It was hereupon moved, that Mr. HOOKE should desire the observation of Sir JONAS MOORE, and insert it in the next *Transaction*.

[1] This account is printed in Mr. HOOKE's *Philosophical Collections*, No. 1, p. 9.

[2] This discourse is published in Mr. HOOKE's *Philosophical Collections*, No. 3, p. 59.

No Spermatozoa seen in Young Cock.

Mr. HOOKE produced and examined the testicles of a cock just killed, but could not perceive any of those small animals in its seed, that had been seen in that of a stone-horse. It was conceived, that the reason was, because the cock was very young, and possibly not fit for generation.

Hooke's Works to be Collected and Printed.

Aug. 7. It was ordered and desired, that Mr. HOOKE do, as soon as may be, print a relation of all the experiments, observations, and relations made and brought in to the society by himself since his first coming into it; and that he have leave to take his own method in the doing thereof.

Transactions.

It was left to him to print the *Transactions*, which he designed to publish, either once a month or once in a fortnight, or oftener.

Correspondence.

It was ordered, that Mr. HOOKE shall proceed with the correspondence, and send away such letters, as are already written; and likewise take care to defray the charge of postage both outwards and inwards.

Vision.

The minutes of July 31 were read: whereupon there was a further discourse about short-sighted persons, and of the ways of vision, from the assertion of Mr. HOOKE, that a man used to see things always inverted would in time judge, that he saw them as they are. Dr. CROONE queried, whence it should come, that the conception should imagine that object erect, which is represented at the bottom of the eye inverted. Dr. GREW supposed, that it might proceed from the   of the optic nerves, which might cause a second inversion. Mr. HOOKE thought, that this could not be the cause, since it was not general in all creatures, and he conceived, that the inversion of the optic nerve was in none observable: but that it rather proceeded from the mind's making comparison of the sensation by the eye with the sensation made by the touch: or rather, that it is an idea or the rule of sight implanted in the soul by nature.

Refraction by Atmosphere.

Mr. HENSHAW observed, that the sun in Norway very often rose and set in an oval figure. Mr. HOOKE affirmed, that it was likewise so here for the most part; which was caused by the refraction or rather inflexion of the air, as he had elsewhere shown.

Apparent Size of Objects.

Mr. HOOKE affirmed, that if you look through a hole a hundred times less than the pupil, the object will appear the same that it does to the naked eye, when it sees it distinctly: but in objects too strong for the eye, it helps the eye by debilitating the rays, which otherwise make a false representation.

Mr. HOOKE conceived, that the object, as the sun, moon, &c. in all probability appeared under the same angle to the eye; but that the judgement or fancy imagined it to appear bigger or less according to the distance, at which it conceived it to appear: and therefore those luminaries are usually conceived bigger, because they are seen to be farther off than the objects nearer of known magnitude: that the refraction is so far from augmenting them, that it rather diminishes them; for should the whole horizon be raised to the zenith, it must appear a point, and always the less, the more it is elevated upward, by reason of the contract of the azimuth: that the imaginary bigness of a sun through a fog arises from the opacity of the air, which is always * * with objects seen at a great distance, or through a great body of air; and by this landscape painters deceive the eye, and make it imagine, that it sees things at a distance, because they are painted with faintish blueness: for a man seen in a fog appears of a gigantic bulk; because, though he be very near, yet being seen through a thick air, the fancy imagines him at a much greater distance.

Remuneration for M. Papin.

Sept. 22. Mr. HOOKE mentioning, that Monsieur PAPIN was suddenly going for Paris, and therefore desired, that in consideration of the time, which he had spent in entertaining the society at their meetings, and in writing letters, he might be considered; it was ordered, that the Treasurer should present him with five guineas; and that if Mr. HILL the Treasurer should not return before Monsieur PAPIN's departure, Mr. HOOKE should pay that money to Monsieur PAPIN, and receive it from the Treasurer.

It was further ordered, that Mr. HOOKE should propose to Monsieur PAPIN in the name of the society twenty pounds a year certain; and that if there could be found a convenient lodging for him in Gresham College, he should be allowed it gratis; and that the society would further study to assist him:

Chelsea Estate.

That Sir CHRISTOPHER WREN, Sir JOHN HOSKYNS, Mr. COLWALL, Mr. HILL, and Mr. HOOKE do, as soon as possible, go to Mr. CHENEY, and view and take possession of such lands, as

belong to the society now lying about Chelsea; and that they give notice to the several tenants of the said lands of such their doings:

Greenwich Instruments to be returned to Gresham College.

That Mr. HUNT take care to have all the instruments of the society now in the custody of Mr. FLAMSTEAD at Greenwich immediately removed to Gresham College; and that Sir CHRISTOPHER WREN and Mr. HOOKE be desired to go thither, and take what care they can in it; and that in the meantime Mr. HOOKE write to Mr. MOORE about the same, and desire to have them carefully sent home; and that the committee meet about this affair on the Friday following.

Papin accepts duty.

Sept. 30. Mr. HOOKE reporting, that upon his making the proposal of the council of the 22nd instant to Monsieur PAPIN of twenty pounds a year certain for writing all letters for the society, he had accepted the same, it was well approved of: but it was further thought fit, that articles should be drawn up to express the conditions expected by the society to be performed by him, and to be subscribed by him.

Nov.

> *Philosophical Collections, containing an account of such Physical, Anatomical, Chymical, Mechanical, Astronomical, Optical and other Mathematical and Philosophical Experiments and observations as have lately come to the Publisher's hands.* [R. Hooke.]

Collection of Arrears.

Nov. 11. Mr. HOOKE was desired to find a fit collector for the arrears due to the society.

Auditing Accounts.

Nov. 20. Mr. HOOKE was nominated on a committee to audit the Treasurer's accounts.

Salary.

It was ordered, that the Treasurer pay to Mr. HOOKE the last quarter's salary ending at Michaelmas past.

Barometer at Altorf.

It was desired, that Mr. HOOKE should write to Professor

STURMIUS at Altorf to request him to keep an account there of the variations of the barometer; which Mr. HOOKE promised to do.

Papin's Air-gun.

Mr. HOOKE gave an account of the trial, that had been made with Monsieur PAPIN'S wind-gun for condensing the air to a sixty-fourth part of the space, which it filled before compression. It was desired by the society, that this experiment might be repeated at the next meeting.

Barometers.

Nov. 27. Mr. HOOKE . . . contrived two kinds of barometers, which were first mentioned to the society, as he conceived, about the year 1662, and lately also, viz. the last year. The first of which should only show the variations of the pressure of the air caused by the alteration of the pressure of the air from either or both causes: but the second should only show the alteration of the pressure of the air from the alteration of the specific gravity.

Oil of Vitriol and Water contract when mixed.

Mr. HOOKE alleged, that he had about eight years before shown the society at Arundel House an experiment to prove the penetration of liquors one into another by putting oil of vitriol into water in a bolt-head of glass; whereby it manifestly appeared, that those two liquors put together took up much less room than when they were separated.[1]

Mr. HOOKE alleged, that there were some instances, whereby it appeared, that bodies really penetrated into the texture of each other, and both together took up less room than they did before they were mixed, and so make a body, that was not only as heavy as it ought to be, supposing these bodies mixed together, but a body heavier than either of them; and consequently there must be a penetration of the texture or dimensions of each other. He alleged also, that there is the like penetration in oil of vitriol and water; and also in divers other bodies, which he could make evident. It was desired, that an experiment of this kind should be shown at the next meeting.

Tin and Copper Alloy.

Dec. 4. The experiment proposed at the last meeting by Mr. HOOKE, to show, that copper and tin being melted together into one mass, would make a composition extremely different

[1] In 17— HAWKSBEE repeated HOOKE'S experiment concerning liquids which when mixed occupy less space than when separate. *Phil. Trans.*, No. 331, p. 325.

from them both, was tried and examined; and it was found, that equal parts of copper and tin melted together made a metal, which was exceedingly hard and very brittle, though the ingredients are both very soft and very malleable: and whereas copper is of a very brown red colour, this was extremely white, and, which was the principal property that it had newly acquired, the specific gravity of it was found to exceed those of both copper and tin; for whereas copper to water is as $8\frac{1}{3}$ to 1, and tin to water as $6\frac{4}{5}$ to 1, and thence the composition ought to have been to water as $7\frac{17}{30}$ to 1; it was found by weighing a part of this substance first in the air and then in the water, that the weight thereof to water was as $8\frac{198}{266}$, or $8\frac{3}{4}$ to 1; for it weighed in the air 2,326 grains, and in the water 2,060 grains.

Hereupon the cause of this was explained by Mr. HOOKE, and ascribed to the penetration, which those bodies made into one another; and it was illustrated by the experiment, that had been formerly produced before the society, of the mixtures of water and oil of sulphur or vitriol.

These experiments were made in order to illustrate some theories about the pressure of the atmosphere, to show how the air might be impregnated with other bodies; whereby the specific gravity thereof might be augmented and altered, the height thereof remaining the same.

Atmospheric Pressure.

Mr. HOOKE alleged several observations made by him, which were contrary to Mr. FLAMSTEAD's view that levity of the air proceeds from its motion; for he had found, that after a long and still rain, during all which the mercury had continued to fall, as soon as the air began to move, or the wind to blow, the mercury began to ascend. It is true, that it often happens, that in great winds the mercury is very low: and so it is likewise when there is no wind at all stirring, as in great rains: and it is no new observation, that in stormy weather the air is light, the barometers being all so marked.

Calcination of Antimony.

Mr. HENSHAW alleged, that antimony by calcining would increase in weight; and mentioned, that Mr. BOYLE had found the same thing. It was therefore supposed, that the gravity of the composition of the tin and copper might be caused by addition to it from the fire.

Mr. HOOKE alleged, that the composition did not weigh heavier than the two ingredients joined together; but rather that it weighed lighter than the ingredients together did before the melting, by reason, that some part of each of them was

wasted by the heat; but that the specific gravity of the composition exceeded the specific gravity of the two joined together as one compound gravity; and not only so, but likewise the specific gravity of the heaviest of them. Besides which he urged, that he had several times calcined antimony by the help of a burning-glass; and had always found it to grow considerably lighter by such calcination.

Newton's Views on Mallemont's Hypothesis of the Heavens.

Mr. HOOKE produced and read a letter of Mr. NEWTON to himself, dated 28th November, 1679, containing his sentiments of Monsieur MALLEMONT's new hypothesis of the heavens; and also suggesting an experiment, whereby to try, whether the earth moves with a diurnal motion or not, viz. by the falling of a body from a considerable height, which, he alleged, must fall to the eastward of the perpendicular, if the earth moved.

Procedure to be followed by the Royal Society.

Dec. 8. It was resolved, that there shall be some one subject fixed upon for the society to proceed upon for the ensuing time, as their main work, till they are satisfied concerning that subject:

That within some reasonable time, as a year, or as soon as they shall be satisfied, that it is brought to perfection, something concerning their progress shall be published:

That in pursuance of this design, some one experiment shall be appointed by the society at every meeting, to be shown at the next meeting in prosecution of that subject so made choice of:

That the several members present, when the experiment is appointed, be desired against the next meeting to consult such authors, as have treated of the said experiment or the subject in debate, and to deliver in what they shall meet with concerning it; and also to speak their own opinion of it:

That the first thing done at every meeting shall be the reading over of the notes of what was done at the preceding meeting:

That a particular and distinct account and narrative of the experiments made at the preceding meeting shall be brought in fairly written by the curator at the following meeting, and be there read before the society, in order to its being entered in the Register, in case the society think fit so to direct:

That whereas it is found, that several experiments made before the society these late years have been only in the Journal Book, and not in the Register; all such experiments shall be forthwith transcribed out of the Journal Book, and being first made perfect and full by the curator, be then fairly entered in the Register:

That whoever is in the chair the last Thursday of the month do call to the Secretary and amanuensis, and see, if as well the

Journal, as the particular experiments, &c. appointed to be entered in the Register, be entered accordingly; and finding them to be so, that under the paper or Journal he write *Entered*:

That likewise at the same time he view the Letter Books, and see, that the letters appointed to be entered be entered accordingly:

That the Secretaries take care to have a small account of philosophical matters, such as were the *Transactions* by Mr. OLDENBURG, and under the same title, published once a quarter at least: and that it be recommended to them to do it monthly, if it may well be; but at least that it be done quarterly.

Mr. HOOKE being asked concerning the undertaking this matter, answered, that he would see what he could do in it, but could not as yet undertake it absolutely.

Experiments to be Prepared.

Dec. 10. Mr. HOOKE was desired to get several experiments ready against the next meeting, by reason that some strangers would be present; and Mr. HUNT was ordered to summon several of the members to be present.

Long Gallery to be Divided.

Mr. HOOKE was appointed on a committee to see, that a partition be put up in the long gallery of Gresham College.

Newton's Letter on Falling Bodies.

Dec. 11. Upon the mentioning of Mr. NEWTON's letter, and the experiment proposed in it, Mr. HOOKE read his answer to him upon that subject, wherein he explained what the line described by a falling body must be supposed to be, moved circularly by the diurnal motion of the earth, and perpendicularly by the power of gravity: and he showed, that it would not be a spiral line, as Mr. NEWTON seemed to suppose, but an eccentrical elliptoid, supposing no resistance in the medium: but supposing a resistance, it would be an eccentric ellipti-spiral, which, after many revolutions, would rest at last in the centre: that the fall of the heavy body would not be directly east, as Mr. NEWTON supposed; but to the south-east, and more to the south than the east. It was desired, that what was triable in this experiment might be done with the first opportunity.

Caves in the Mendips.

Mr. HOOKE read an account, which he had procured from Mr. BEAUMONT, of several observations made by himself in divers subterraneous caverns in Somersetshire, viz. in Wookey Hole, in a cavern near Cheddar, and in the hill called Lamb, above

the parish of Harptree among Mendip Hills; which account Mr. HOOKE intended to print the first opportunity.[1]

Mr. HOOKE mentioned, that he had received a letter from Mr. WILLIAM BALLE, and another from Dr. BEAL; but it being pretty late, the society desired to have an account of them at another time, and rose to observe the experiments.

Weight of Alloys.

1. An account was given by Mr. HOOKE of the experiment for examining the weight of tin, copper, and the mixture of tin and copper, and the way and reason thereof explained.

Inspection of Letters.

Dec. 17. Mr. HILL, Dr. GALE, Dr. CROUNE and Sir JOHN HOSKYNS were appointed a committee to meet at Mr. HOOKE's lodgings on the Monday following, to see the letters in his custody.

It was ordered, that a list be made by the Secretaries of all letters, that had come to their hands during the whole year; and that at the end of each year such a list be delivered to the President.

Philosophical Collections.

Mr. HOOKE was desired to continue the *Philosophical Collections.*[2]

£32 for an Assistant.

Mr. HOOKE propounding, that it was necessary for him to have a person, who might be constantly by him, and employed in the making and preparing of such trials and experiments, as should, when perfected, be shown and represented to the society at their meetings; it was ordered, that he should have liberty forthwith to employ such person, as he should agree with; and that the society would allow for the hire of such person thirty-two pounds per annum.

[1] *Philosophical Collections*, No. 2, p. 1, 1681.

[2] The first number was printed at London, 1679, in 4to, by Mr. MARTYN, printer to the society, under this title: *Philosophical Collections, containing an account of such physical, anatomical, chemical, mechanical, astronomical, optical, or other mathematical and philosophical experiments and observations, as have lately come to the publisher's hands. As also an account of some books of this kind lately published.* The second number was printed at London, for MOSES PITT in 1681; the third for R. CHISWELL, and dated December 10, 1681, with an advertisement, that these *Collections* would for the future be published once a month at least; wherein would be contained an account of all such new discoveries of nature or art, as should occur to the collector in the modern books or writings of learned men, either at home or abroad. The fourth number is dated, January 10, 1681/2, the fifth, in February, 1681/2; the sixth, in March, 1681/2; and the seventh and last in April, 1682.

Newton's Letter and Hooke's Experiment on Falling Bodies.

Dec. 18. Mr. HOOKE read his answer to Mr. NEWTON's former letter; as also another letter, which he had received from Mr. NEWTON, containing his further thoughts and examinations of what had been propounded by Mr. HOOKE.

Mr. HOOKE gave also an account, that he had made three trials of the experiment propounded by Mr. NEWTON, and had found the ball in every one of the said experiments fall to the south-east of the perpendicular point, found by the same ball hanging perpendicular. But the distance of it from the perpendicular point being not always the same, and the experiment having been made without doors, in the open air, nothing of certainty could be concluded from it. But he alleged, that he designed to make a trial of it within doors, where there would be less motion of the air; and he hoped to be able to do it before the next meeting of the society.

Bartholine desires to correspond.

Mr. HOOKE read a letter of ERASMUS BARTHOLINE, which he had received from the President, directed to Dr. GREW, and dated at Copenhagen, February 23, 1679; in which the writer expressed his readiness to correspond, and enclosed a treatise of his nephew, CASPAR BARTHOLINE, *De Organo Olfactus*; which treatise was delivered to Dr. CROONE to peruse and give an account of it at the next meeting, in order that an answer might be sent to the letter.

Trisection of Angle.

Mr. HOOKE showed a new book of Signor VIVIANI, sent to himself by the author, containing several solutions of the problem propounded by Mr. CONYERS of trisecting an angle, and finding two mean proportionals. Two other copies were likewise sent, one for the President and the other for Mr. COLLINS, which were accordingly delivered.

1679/80

Theory of Circular Motion.

Jan. 8. Sir CHRISTOPHER WREN Vice-President in the chair.

Mr. HOOKE read another letter of his to Mr. NEWTON concerning some further account of his theory of circular motion and attraction; as also several observations and deductions from that theory; as 1. That pendulum clocks must vary their velocity in several climates. 2. That this variation must also happen at different heights in the same climate: Which last remark he confirmed by an observation of Mr. HALLEY at

St. Helena; and 3. As a consequence of these, that a pendulum was unfit for a universal standard of measure.

Mallemont's Hypothesis.

Mr. HOOKE read also a letter of Mr. BALLE's, giving an account of his thoughts concerning the hypothesis of Monsieur MALLEMONT.

Camphor and Nitric Acid.

Mr. HOOKE showed an experiment of the putting of camphor into aqua-fortis, whereby that gum is presently converted into oil, and swims in that form upon the aqua-fortis.

Earth's Diurnal Motion.

He was desired to make his trials as soon as possible of Mr. NEWTON's experiment concerning the earth's diurnal motion.

Density of Tin and Lead.

The experiment to try the comparative weight of tin, lead, and the mixture of tin and lead was prepared; but it being late it was referred to the next meeting.

Camphor and Nitric Acid.

Jan. 15. The minutes of January 8 were read; which gave occasion to discourse about the experiments made at the last meeting with camphor and aqua-fortis.

Mr. HENSHAW related, that camphor so liquified had been found a certain and speedy remedy for the toothache, arising from hollow teeth.

Dr. GREW supposed, that it might be from the acidity given it by the aqua-fortis; for that all acids very much contribute to the fastening and strengthening of the teeth. Mr. HOOKE supposed, that a moderate acid might serve to cleanse the teeth, if foul; but that the acidity of aqua-fortis was much too violent; and he conceived, that it would rather damage the tooth itself, and so leave it more subject to external injuries from acids or other noxious qualities; and that it is a general observation, that after eating sharp and acid juices the teeth will for some time be very sensible and tender, and be much more affected with sharp or sweet juices, or with hot or cold substances, than at other times.

Microscopic Structure of Wood.

Mr. HOOKE produced the translation of a long letter, which he had received from Mr. LEEWENHOECK, written in Low Dutch; together with several curious drafts of small pieces of wood observed in the microscope; as also the letter itself. A part of this translation was read, and the delineations examined, wherein

were explained the several vessels and curious contexture of the parts of wood. The remaining part was referred to the next meeting.

Specific Weights of Tin and Lead.

The experiment showed was the examination of the specific weights of tin, and a mixture of equal parts of lead and tin; which was done by weighing them first in the air, and then in the water, and taking notice of their particular gravities. The weights of them were as follow:

	before melting	in air	in water	the difference		
Tin	3,840	3,536	3,044	492	$7\frac{92}{491}$	$7\frac{2}{11}$
Lead	3,840	3,628	3,266	362	$10\frac{4}{181}$	$10\frac{1}{25}$
Tin and lead	7,680	7,096	6,120	976	$7\frac{33}{122}$	$7\frac{3}{11}$
						$8\frac{23}{61}$

Worms in Ginger-water.

Jan. 22. Mr. HOOKE read a letter, which he had received from Mr. LEEWENHOECK, giving account of some further discoveries of an exceeding small sort of worms found in ginger-water; as also the reasons, why he conceived, that the parts of water cannot be made visible by a microscope.

Phosphorus.

Mr. HOOKE produced a phosphorus given him by Dr. SLARE, which was examined by Mr. HENSHAW, Sir CYRIL WYCHE, and several others, and was found to be very receptive of light.

Falling Bodies.

Mr. HOOKE showed the ball, that had been let fall from the height of 27 feet, and fell into a box full of tobacco pipe-clay, sticking in the clay, upon the surface of which were made lines crossing each other: which showed the true perpendicular point indicated by the ball, when it hung suspended by a thread from the top, and how much the ball had varied from that perpendicular in its descent towards the south and east: and he explained the manner, how the same was performed in all particulars. It was desired, that this experiment might be made before a number of the society, who might be witnesses of it before the next meeting. The time appointed was the Monday following at three in the afternoon.

Specific Weights of Copper and Lead Mixture.

The experiment of weighing the mixture of copper and lead was tried by examining its weight both in air and water; and thereby it was found, that the said mixture in the air weighed

4,188 grains; in the water 3,746 grains; whence the weight of it to that of water was as $9\frac{105}{221}$, to one, or $9\frac{21}{44}$ to one.

Dr. Heusch.

Dr. CHRISTIAN HEUSCH, principal physician to the elector Palatine, who was present at this meeting, was proposed candidate by Mr. HOOKE.

Jan. 23.

LETTER FROM HOOKE TO LEEWENHOECK.

Worthy Sr. I have received the favour of your masters last letters and having translated them into English, I have communicated them to the R. S. at their public meetings, who were extremely pleased with the great curiosity of your delineations and descriptions and desired me to return you their hearty thanks for your so freely and fully communicating to them what discoveries you make with your microscope. They are much surprised with that discovery which you have made of small animals in the sap which runs from trees, and would be glad to be further informed whether you have met with them in the juice of any other vegetables or the parts of them as in fruites, flowers, leaves, roots, etc. There has been some observations made at their meetings of the small animals in *Semine Animalium* whereby vast numbers of those little creatures were made very visible in the liquor taken from the testicle of a Stone Horse, but they have not as yet examined the melt of Fishes. My other occasions have hitherto hindered me from making such tryalls though I determine within a little time to make some observations of that kind, of which I will give you an account. I readily concur with you in the opinion of the exceeding smallness of the parts of water and that it will be exceeding difficult to detect them by the microscope, though yet methinks there seems to be a possibility of discovering the coloured parts of liquors they are very visible in ink and several other opacous coloured liquors where the colour is made by a kind of precipitation (as the chymists speak) where the coloured parts of the liquor are as it were, thrown out by the liquor into distinct parcells and lumps, which just float up and down in it, being still much of the same gravity; but whether microscopes will help us to distinguish the parts of water I yet doubt. I wonder you did not receive the letter I sent with the collections. I delivered them to a merchant who promised to get them safe conveyed to you. I doe much wonder that your name is not in the list of the Royal Society, especially since I find Mr. Oldenburgh received the favour of soe many excellent communications from you. If I thought it would be grateful to you I would propound you at the meeting as a candi-

date. If you please to let me know your thoughts of it by your next I shall regulate myself accordingly, and give you a speedy account thereof. There will be nothing of charge to you upon that account, and I doubt not of effecting it, if you desire it. I am indeavouring to get the Delineations of your former letter graven in order to have them printed, of which I may give you shortly a further account. In the meantime I remain Sr.

Your humble servant, R. H.

Jan. 23, 1679. [MS. Sloane 1039, f. 172.]

Worms in Ginger-water.

Jan. 29. The minutes of the 22nd instant were read; and upon discoursing about the small creatures discovered by Mr. LEEWENHOECK in ginger-water mixed with pepper-water, it was ordered, that some should be prepared against the next meeting.

Specific Weight of Alloys.

Mr. HOOKE read an account of the experiments, which had been shown to the society of the comparative weight of two mixed metals with the weight of the several metals, out of which they were compounded; from which he deduced, that the invention of ARCHIMEDES to find the cheat of the goldsmith in making HIERO's crown, though in itself very ingenious, might not be sufficient to perform what he designed by it; for that some two metals compounded made a heavier, and some other two a much lighter, than they really ought.

'On Specific Gravity read in ye Society Jan. 29.' MS. *Sloane* 1039, f. 114.

Alloys.

Feb. 5. Mr. HOOKE gave an account of some other qualities, which he had taken notice of in the mixture of tin and copper, as: 1. That the colour of the copper was quite destroyed, it appearing much of the colour of iron, when polished. 2. That the composition, which was made of two very malleable metals, when mixed, became friable and brittle. 3. That it bore a pretty good polish and reflection. 4. That though copper is exceedingly hard to be melted, yet this mixture melted very easily. 5. That viewing the polished surface of it with a glass, he found it very full of extremely small holes or blebs in the metal.

Mr. HOOKE read an account of the experiment, which was made the Monday before by Mr. HILL, Dr. CROONE and Mr. HOOKE, which was ordered to be entered into the Register, as follows:

February the 2nd, 1679/80; the several weights of lead and tin,

and a mixture made of equal parts of lead and tin melted together, were examined; and the proportions taken as followeth:

Lead, weighed in the air, was . . .	3,450 grains.
Tin, weighed in the air, was . . .	3,450 grains.
Lead, weighed in water, was . . .	3,138 grains.
Tin, weighed in water, was . . .	2,988 grains.

The aforesaid equal parts of lead and tin, being melted into one mass in a crucible, and being then examined, we found that,

This mixture weighed in air . . .	6,876 grains.
The same weighed in water was . . .	6,078 grains.

Whence the specific weight of the metal compared with water, was $8\frac{28}{113}$.

The specific weight of lead, as above . .	$11\frac{8}{104}$
,, ,, of tin ,, . .	$7\frac{31}{88}$
Whence the specific weight of this mixture should have been	$9\frac{1810}{8864}$

It was desired, that trials should be made, between that and the next meeting, with mixture of tin and copper, tin and silver, silver and lead, and silver and copper: and that the time of making some of them should be on the Monday following at three in the afternoon; and that any member of the society, who pleased, might be present.[1]

Justel's Automata.

Mr. HOOKE produced a letter from Monsieur JUSTEL, which he had received that afternoon, though dated at Paris, December 23, 1679. It was read by the President. It contained an account of an artificial man and an artificial horse, and of a machine for transporting earth.

Bermudan Fruit.

Dr. HOOKE showed an Indian fig or prickly pear from Bermuda, which was opened, and the deep red juice thereof tasted by several of the members.

Leewenhoeck's Letter.

Feb. 12. A letter from Mr. LEEWENHOECK to Mr. HOOKE, translated by Mr. ASTON, and dated at Delft, February 13, 1680, N.S. was read, acknowledging the receipt of the last letters and books sent him, and expressing his desire to be chosen a member of the society; and mentioning, that he was busy in making two observations, which he promised to transmit to the society.

[1] An Account of some Trials made on the mixture of Metals, in f. 120 of MS. *Sloane* 1039.

Silver-tin Alloy.

Mr. HOOKE read his account of the experiment, which had been made on the Monday preceding, of melting silver and tin together:

Monday, February 9, 1679/80, half an ounce of refined silver (the specific gravity whereof, to water, was found to be as $9\frac{2}{3}$ to 1) was melted in a crucible in a charcoal fire, in about half a quarter of an hour; and then an equal weight of block-tin was thrown into it, which immediately melted like butter, but cooled the silver. Blowing the coals again, the whole mass melted and incorporated, but there seemed a great deal of dross at the top. Then we took out the crucible, and suffered it to stand till it was almost cold: then turning up the crucible upon a sheet of paper, a great deal of powder and scoria and some pieces of metal, which though it were so cold as not to burn the paper, yet was it so soft, as to temper and spread with the point of a knife like an amalgam of mercury and tin: but breaking the crucible, we found a pretty quantity of metal at the bottom. We picked out as much as we could of the metal, and to make it in a lump, we melted them in another crucible, which it presently did, and poured it into water: this lump of metal we weighed, and found it to weigh in the air 173 grains, and in the water 157 grains; whence the specific gravity thereof is $10\frac{13}{16}$, somewhat more than $10\frac{3}{4}$. So that by the mixture of an equal weight of tin, whose specific gravity to water is but as 7 to 1, the silver is made above the tenth part heavier.

The metal being shown to the society, and by a blow of a hammer broken before them, it was observed to be very heavy, close, hard and brittle.

The experiments of gold and silver, and lead and silver, were desired to be made on Monday following at two in the afternoon in the repository.

Feb. 19. Mr. HOOKE read an account of some experiments made on the Monday preceding upon a mixture of silver and lead; which account was ordered to be entered in the Register Book, and was as follows:

Monday, February 1679/80. We found by trial, that the specific weight of silver was $9\frac{2}{3}$, and that the specific weight of lead, examined by water, was $11\frac{2}{3}$; and by melting half an ounce of silver and half an ounce of lead together, and weighing the mixture first in air, then in water, we found the weight of it in the air to be $439\frac{5}{10}$, and the weight of the same in the water to be $397\frac{4}{10}$: whence we deduced, that the specific gravity thereof was $10\frac{3}{7}$: and by comparing it with what it would have been, supposing an equal mixture without penetration, we found it

should have been $10\frac{2}{3}$, so that it was found somewhat lighter than the medium of their two specific gravities, which we supposed to be caused by the wasting of some part of the lead into litharge. So that it seems in this composition the metals do not work upon and penetrate each other, as in some of the other mixtures.

Then with a hammer and anvil we beat this mixture very flat, to see if by the strokes of the hammer they could be made to penetrate each other, or to lie in a closer texture: but weighing the piece so hammered, first in air, and then in the water, we found the weight thereof in both cases the same with the former weight. It was very malleable, and seemed of a middle nature, as to hardness and malleableness, as it did also as to its colour between silver and lead.

After this we put the same lump into the same crucible, and suffered it to stand in the fire melted, till all the lead was thrown out of the silver into a litharge, which remained melted at the top, and was converted into a glass, somewhat yellowish but transparent, and which was very brittle. But it was observable, that so long as there remained any lead with the silver, it kept the same in fusion, though the fire was not very strong: but so soon as it was all boiled out into litharge, the silver grew hard, though the litharge remained melted into a glass; which we poured out into a cake.

Examining the comparative weight of this glass of litharge to water, we found the specific gravity thereof to be $6\frac{3}{25}$, that is near $6\frac{1}{8}$. So that lead reduced into glass hath acquired a texture twice as much rarefied, as it was whilst a metal.

Examining the specific gravity of a lump of glass between white and green, we found it to be to water as $2\frac{1}{2}+\frac{1}{32}$: so that this glass of lead is almost three times as heavy as common glass.

The experiments to be tried on the Monday following at three in the afternoon were appointed, viz.: 1. With silver and gold. 2. With silver and copper; and 3. With brass and lead.

Boyle's 'Sceptical Chemist'.

Feb. 26. Mr. Hooke presented from Mr. Boyle his *Sceptical Chemist*, lately reprinted by him with many additions; which treatise the committee for experiments were desired to peruse, and to give an account of to the society, and to see what experiments contained therein were proper to be shown at the meetings of the society.

Experiments of Weighing and the Pendulum.

They were desired by Mr. Hooke to meet upon the Monday following in the afternoon, when he designed to try some experiments of weighing.

Mr. HOOKE mentioned, that one part of the design, viz. the taking off all inequality of force from the pendulum, had been long since completed and shown by himself before the society, when they met at Arundel House, as would appear from their Registers: That this author had only asserted, that there was such a way, but had not shown what his way was: and that some other of the ways, which he had there proposed, appeared to be much inferior to those already practised.

Mr. HOOKE gave an account of the trials made upon the Monday preceding; as likewise of the method, which he had made use of to adjust the weights for succeeding experiments by dividing extended wires, &c. into grains, half-grains, quarters, and eighths: And he mentioned, that he would by those examine the weight of gold, silver, copper, tin, lead, &c. and on the Monday following make the trials.

Alloys. Metal Draw-plates.

Hereupon several discourses were occasioned about the nature and temperature of simple and mixed metals; and particularly of the metal, of which the holes, through which wire is drawn, are made; which, Mr. HOOKE said, he had been informed, was made by one man only, and not known to any other person.

It was desired, that one of those plates with holes should be procured against the next meeting.

Hair on a Corpse. Silk.

Upon mentioning the observation, that the hair was first very soft, and afterwards hardened in the air, Mr. HOOKE remarked, that the   of silkworms, spiders, caterpillars, &c. which in the body were soft, so soon as they were drawn into the air, hardened into a thread.

Dip-needle.

Mr. HOOKE objected, that though a dipping-needle would vary from the meridian, according to various inclinations in several azimuths, as he had long since found, and reduced to a theory; yet he conceived, that the leading or poising a horizontal needle to such an inclination would not have the same effect; and therefore he doubted, whether that would succeed.

Swammerdam on Insects.

Mr. HOOKE produced a large discourse about insects, being a translation of the principal things contained in Dr. SWAMMERDAM's book. But it being now eight o'clock at night, the society rose.

Philosophical Gazette.

Feb. 28. At a Meeting of the Council at which Mr. HOOKE was

present, the heads of the *Philosophical Gazette* were discoursed of, and some of them set down. Mr. HOOKE was desired to make a trial of one.

Feb. 28. At a meeting of the Council it was "Resolved that Mr. Hooke be desired to publish (as he declares he is now ready to do,) a sheet or two every fortnight, of such philosophical matters as he shall meet with from his correspondents; not making use of any thing contained in the Register-books, without the leave of the Council and author."

Thus originated Hooke's celebrated *Philosophical Collections*, of which seven numbers were printed in 1680–2. They gave *Accounts of Physical, Anatomical, Chymical, Mechanical, Astronomical, Optical, and other Mathematical and Philosophical Experiments and Observations.*

1679–80. "Resolved, that, in consideration of propositions made by Mr. Hooke for a more sedulous prosecution of the experiments for the service of the Society, and particularly the drawing up into treatises several excellent things which he had formerly promised the world; the Council, as an encouragement, according to the poor abilities of the Society, have agreed to adde forty pounds for this year, ending at Christmas, to Mr. Hooke's salary."

Metal of Plate for Drawing Wire.

Mar. 4. Mr. HOOKE gave an account of the same metal; that it could be both hardened and softened by heating and quenching: That it was supposed to be some preparation of steel: That the great use of it was for drawing gold, silver, and copper wire, &c. That steel wire could be drawn through plates made in England of steel not hardened; but that these plates were apt to fret the wire of softer metal: That he would make some trial of this metal, by which he hoped he might give some more certain account what the metal was; which he was desired to do.

Polishing Earth.

Mr. HOOKE inquired, whether any person present could give any information concerning a certain English earth very effectual for scouring copper, brass, &c. but none having before heard of it, the members were desired to inform themselves further concerning it: And Mr. HOUGHTON promised to inquire of some, who, he thought, could inform him.

Copper and Silver.

The trial on the Tuesday following, promised by Mr. HOOKE, was of copper and silver.

Dip-needle.

Upon the objections made by Mr. HOOKE to the inferences made at the last meeting by Mr. PERKINS from his experiments showed with a dipping-needle variously inclined in the north and south azimuths that the compass needle would also vary, if the south end thereof were made to dip; Mr. PERKINS explained what he meant by the north end of the dipping-needles pointing southwards beyond the equator.

Mr. HOOKE also explained those experiments, and showed, that there was nothing in them, which varied from the known magnetical rules; and showed the reason of the directing of the dipping-needle towards other places than it would naturally do, if it were not limited and restrained: and that in this restraint it did, as near as it could, place itself in its most natural posture: That this had no manner of influence upon the common compass needle, whether it hung horizontal, or whether the south or north end dipped below it. And because there were some misunderstandings about the names of the north and south ends of the needle, Mr. HOOKE explained what he meant by pointing northwards and southwards, viz. towards any point of an hemisphere, or the north or south side of the east and west azimuth; which he made more intelligible by a draft, whereby he showed also the manner of the dipping of the needle below the horizon.

Whistler's Rarities.

Rarities procured by Mr. HOOKE from —— WHISTLER, Esq.; and presented to the society for their repository, and delivered this day to Mr. HUNT.

Gold-silver Alloy.

On this day, there was read and entered into the Register the following account of an experiment of the weight of gold and silver melted together, by Mr. HOOKE:

Monday, *March* 1, 1679/80. The weights having been before, with very great care and curiosity, adjusted, so as to be all in a true proportion to one another, and everything being well adjusted and fitted for the trials; we examined the weight of the gold (which was of the best refined water-gold) and found the same in the air to be counterpoised by . . $109\frac{1}{4}$ grains.

In the water it was counterpoised by . . $103\frac{1}{2}$ grains.

The specific gravity as to water was as 19 to 1.

We examined the weight also of refined silver by the same weights, and found it in air to be . . . $109\frac{1}{4}$ grains.

And in water . . . . $98\frac{3}{4}$ grains.

Whence the specific gravity to water is as $10\frac{17}{42}$ to 1.

Then we melted them together and let them cool, and weighing the mass in air, we found it . . . . $215\frac{1}{2}$ grains.

And in water . . . . . . $199\frac{1}{2}$ grains.

whence the specific gravity to water was as $13\frac{15}{32}$ to 1. But the medium of the specific gravities of the gold and the silver was to water as $14\frac{3}{4}$ to 1. Therefore the mixture was lighter than it ought to have been, according to the supposition of ARCHIMEDES by $\frac{2}{23}$, that is somewhat more than a twelfth part.

Then with a chisel we cut the lump in two, and found the middle part of it look pretty yellow like gold, as if the gold had not been all perfectly mixed with the silver, though the silver encompassed it. We melted it therefore again in the same crucible, and when cool, weighed it and found its weight in the air $215\frac{1}{4}$ grains, and in the water $199\frac{1}{4}$ grains; whence its specific gravity was much the same as before, viz. $13\frac{29}{64}$ to 1.

Then we cut the lump in sunder with a chisel as before, and found, that the mixture was perfect, and the colour of the gold wholly lost.

Mar. 6. HOOKE wrote to Rev. J. BEALE of Somerset.

[MS. *Sloane* 1039, f. 171.]

Rotten-stone.

Mar. 11. Mr. HOOKE gave an account, that upon his further inquiring concerning the material used for scouring and polishing metals, as brass, copper, &c. he had been informed, that it was called rotten stone; and that it was brought out of Lancashire, Derbyshire or Cheshire, or that way; but the particular place he could not yet be informed of. Some of it was produced, and seemed to be a light hard and gritty earth.

Alloy.

The account of the experiments tried on the Tuesday preceding was brought in by Mr. HOOKE and read, being examinations of the weight of a mixture of silver and copper, as also of a crown-piece; which account was ordered to be registered.

The experiments for the Tuesday following propounded by Mr. HOOKE were appointed to be on the mixtures of iron and tin, and iron and lead.

Magnetic Variation.

Mr. HOOKE gave an account, that he had lately written to Mr. WILLIAM BALLE, to inquire concerning the present variation of the needle in Devonshire: and he was desired, when he wrote next to Mr. BALLE, to request an account of what experiments of that kind he had formerly made.

South Sea Coasts.

Mar. 18. Mr. HOOKE mentioned the description of the South Sea coasts made for the King of Spain; and presented to His Majesty, in the possession of the Earl of Bristol at the time of his death, as he had been informed; but not heard of since that time.

Specific Weights of Metals.

On this day, though there is no entry of it in the Journal, there was given in by Mr. HOOKE the following account of the weight of several metals:

Tuesday, March 16, 1679. We first examined the weight of regulus of antimony in air and water, and found the weight to be in air $135\frac{1}{2}$, in water $115\frac{3}{4}$: whence the specific gravity is $6\frac{68}{79}$.

We found also the weight of common iron in air 697, in water $606\frac{1}{2}$, whence the specific gravity $7\frac{127}{181}$.

The weight of the wire drawing-plate was in air $465\frac{3}{8}$, in water $399\frac{1}{8}$, whence its specific gravity is $7\frac{13}{530}$ or $7\frac{1}{41}$.

The weight of the gold-coloured mixture, supposed to be made of spelter and copper, was found in air 1,317, in water 1,166: its specific gravity $8\frac{109}{151}$ or $8\frac{2}{3}$. Another piece examined, was found in air 1,296, in water $1,146\frac{1}{2}$: therefore its specific gravity $8\frac{200}{299}$ or $\frac{2}{3}$.

The mixture of antimony and iron did not succeed.

1680

Lectures on Light and Geometry.

In the beginning of 1680 Mr. HOOKE began a course of Lectures on Light. These, and also a specimen of his *Geometrick Lectures* delivered in the same year, have been printed in his *Posthumous Works*, pp. 65, 71 et seq.

Straits of Magellan unfathomable.

Mar. 25. Mr. HOOKE remarked, that he had been informed by Captain WOOD, that upon sounding in some parts of the Straits of Magellan with 1,000 fathom of line, he could find no bottom.

Digestion.

Mr. HOOKE was of opinion, that a great cause of the dissolution by the stomach was a continual motion of the stomach, whereby the food therein contained was continually squeezed and churned; by which means concoctions, dissolutions, or digestions were caused, which without that motion could not be effected, either by the heat or menstruum of the stomach.

Specific Weights of Metals.

The account of the experiments made on the Tuesday before

by the committee for that purpose, was brought in by Mr. HOOKE, and read as follows:

Tuesday, March 23, 1679/80. We made a regulus of equal parts of antimony and iron, and found it to weigh in the air 533 grains, and in the water $457\frac{1}{4}$: whence the specific gravity is $7\frac{11}{103}$. This we found to be very hard, but yet brittle and not at all malleable. It broke with a short grain and black, or little reflective: it seemed not very likely to take any polish. It had no manner of operation on the magnetic needle.

This part we melted with equal parts of tin, and found a mixture, that looked pretty white: it continued melted with a small heat, little more than red-hot. It broke into several pieces with two or three blows of a hammer; the grain of which was exceeding fine, close and smooth; and whiter than bell-metal. We polished it, and found it held a very good polish, which gave a strong reflection. Its weight in air was $859\frac{1}{2}$, in water, $738\frac{1}{4}$, whence its specific gravity is as $7\frac{43}{485}$. We conceive it may be very useful for making speculative glasses for Mr. NEWTON's experiment.

Then, with another part of the regulus of antimony, we mixed an equal weight of lead, which soon incorporated together: the result was, that it made a compositum very hard and very brittle. Its weight in air was $422\frac{6}{8}$, in the water $365\frac{5}{8}$: whence its specific gravity must be as $7\frac{182}{457}$.

The President moved, that against the next meeting Mr. HOOKE should make a table of all the experiments of this kind, that he had already tried and designed further to try, that so that subject might be brought to a conclusion, and another be pitched upon.

The experiments appointed to be tried on the Tuesday following were mixtures of tin, lead, and regulus martis with brass.

Digestion.

Apr. 1. Mr. HOOKE being asked some reason, why he supposed the stomach to promote digestion by its motion, alleged the muscular composition and make of the stomach, especially remarkable in the gizzard of fowls; and the motion, which he had observed of it in divers insects, which are transparent; as also the peristaltic motion of the guts.

Leewenhoeck's Letter.

Mr. LEEWENHOECK's letter to Mr. HOOKE, who had translated the sense of it into English, was read, giving an account of some further discoveries of his about the eel-like worms in the seed of a rat; as also of the motion of the gills of mussels, oysters, &c.

Alloys.

Mr. HOOKE produced an account of the experiments made at the last day; as also a table of all the mixtures of metals, that were at first designed to be tried; but no further trials for the next meeting were fixed upon.

Apr. 6.

DRAFT OF LETTER FROM HOOKE TO J. C. STURMIUS OF ALTORF.

Worthy S^r^. Your most obleiging letter of the 10th of Feb.: I received and communicated the contents thereof to the Royall Society at their publicke meeting,[1] with which they were very well pleased, and desired Dr. Gale (who hath undertaken for the future to keep the forreign correspondence) to send you their thanks for your ingenious communications and to desire you for the future to continue them, and also to communicate unto you what doth here occur new in philosophical matters, which you may be sure he will accordingly perform, as I should have done if I had taken that care upon me. But I have another Province committed to my care, however, Sr., I shall not be wanting when I meet with anything very singular to give you an advertisement thereof either by my own letter or that of my brother Secretary Dr. Gale.

Your advertisement concerning the variation of the magneticall needle came very opportunely even when we were upon that very inquiry, and we had newly made the observation of the variation at Greenwich about 4 miles east of London and found it about 4½ degrees to the westward of the north, and I have sent to severall correspondents in other parts of England to observe the same and send us an account thereof which if you desire, soe soon as we receive, you shall have sent you.

We have ye phospho: Baldwini, pray send that of Kunkell.

Mr. Boyle engen book not published, of Pappin windgun conden 64 times holding seuerall months—rarefying engine a whole year. Beckers two books. A wish of Prosperity

[R. H.]

[MS. *Sloane* 1039, f. 173.]

Leibnitz's Letter.

Apr. 15. The first was a letter from Monsieur LEIBNITZ to Mr. HOOKE dated in February 1679/80, at Hanover, giving an account of his Prince's design of making a survey of his own country; inquiring concerning the undertakings of Dr. PELL, and especially his way of resolving equations by a table of signs; answering some propositions made to him by Mr. HOOKE, as

[1] Mar. 11.

particularly about his arithmetical instrument and the late Bishop of Chester's universal character and language; hinting an invention of his, which he supposed much more useful for the improving reason; mentioning, that he had by him *Vas arcanum luminis perpetui* different from all other shining bodies; and inquiring concerning Mr. BERNARD, and whether our manuscripts contain more of APOLLONIUS than what BORETTUS had printed.

Upon discoursing concerning Monsieur LEIBNITZ's new invention, Mr. HOOKE remarked, that he had an invention of that kind in several other subjects besides geometry and arithmetic.

Ludolf's Letter.

The third letter was from Mr. JOB LUDOLFUS to Mr. HOOKE, dated at Frankfort, December 31, 1679, giving an account, that Dr. CLAUDER's invention of preserving dead bodies was in the press; and that himself would for the future gladly continue his correspondence, and communicate what he should meet with considerable with regard to geography.

Glass Beads.

Apr. 22. Mr. HOUGHTON produced a blue bead much esteemed by those of Guinea; as also some glass beads, that had been made to counterfeit the colour: but they were in no respect so beautiful and clear of colour as those of Guinea. One of the latter was tried to be melted in the flame of a lamp, but without success; but it flamed and cracked like a stone. The counterfeit one melted very readily.

Mr. HOOKE conceived, that he could make glass beads as beautiful for colour, but could not imitate the hardness and fixedness of them. He was desired to make some trials about it.

Some of Dr. BEAL's letters[1] to the Secretary were read, and the rest reserved to the next meeting.

New Level.

Mr. HOOKE produced and showed a new kind of level invented by himself, it being the most convenient and exact way of any, that hath yet been made use of. The instrument not being quite perfected, the description thereof is omitted till the next meeting.

Atlas.

Apr. 25. MOSES PITT of Oxford wrote to HOOKE a '*Design for a representation of the Universe*' in a new Atlas. A sketch of the

[1] In the Letter Book, vol. viii, pp. 95, 107, are three letters of Dr. BEAL to Mr. HOOKE, one dated February 18, 1679/80, about grapes thriving in England; another dated March 13, on the same subject; and the other March 31, 1680, concerning the improvement of land by rain, and about cherries and cider.

Method to be observed, and also the printed *Proposals* are preserved *in extenso* in MS. *Sloane* 1039, ff. 1–37.

Terrestrial Magnetism.

Apr. 29. A letter of WILLIAM BALLE, Esq. to Mr. HOOKE, dated at Mainhead in Devonshire,[1] April 24, 1680, was read, giving an account of some magnetical observations, which he had already made, and of his intentions of making divers other instruments and trials; as particularly of making a magnetical needle of 10 feet in length, and another of 20 feet in length, in order to examine the variation of the directive virtue. Mention was likewise made in this letter of an observation of the variation near the Sound.

Hereupon Dr. WALLIS mentioned some opinions about the variation of the latitude of places. And Mr. HOOKE remarked, that Monsieur PETER PETIT had written a discourse on that subject, endeavouring to make it probable. It was conceived, that the cause of this opinion might be imperfect observations made of the latitude of places by different authors; and that till there was more certainty of the accurateness of instruments and observations, nothing could be concluded about that controversy.

Justel's Letter.

A letter of Monsieur JUSTEL to Mr. HOOKE, dated at Paris, April 10, 1680, giving an account of a German physician, who distilled the matter of a plague sore; of Monsieur VILLETTE's making a burning-glass of three feet seven inches diameter, the focus being 3½ feet distant; with some other articles of literary intelligence.

New Level.

Mr. HOOKE produced his new level, and explained it; and showed wherein he conceived it to be superior to any kind of level yet made for plainness, certainty, and exactness.

Micro-organisms in Mint Water.

May 13. After an exhibition of Mr. MELLIN's small lenses for the microscope, Mr. HOOKE gave an account, that he had seen several small animals in the water, wherein mint grew, the water was examined with a microscope, and several of those small long creatures were discovered by the Vice-President and divers of the members.

Metals. Speculum Metal.

Mr. HOOKE said, that there was a way of making some mixtures of metals (as the factitious gold made with spelter, which is of

[1] ? Minehead in Somersetshire.

itself very brittle) very tough and malleable, by cementing it with a vegetable powder. And that there was a way of hardening an amalgam of mercury and iron by a vegetable powder, which would make it almost as hard as hardened steel. This, he conceived, would be an excellent material for making specular planes for telescopes in Mr. NEWTON'S way; since the form of such plates would be easily given by laying the said amalgam, when soft, upon the convex side of an object glass for a telescope made very large. The composition and manner of making and hardening that amalgam was much desired.

Mercury.

Mr. HOOKE mentioned, that he had been lately informed of a way to harden and fix mercury; but that he had not yet tried it, to see, whether it would succeed.

May 19.

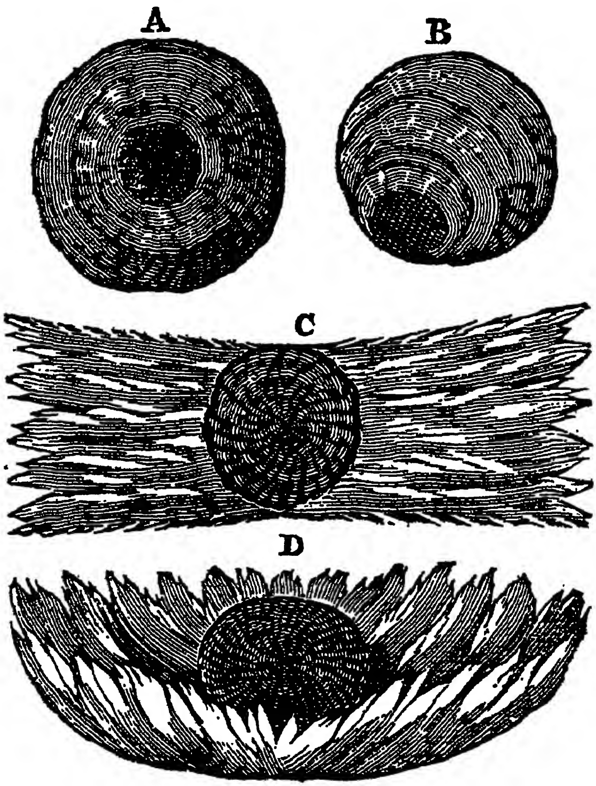
Account of the great hailstones that fell in London on May 18, 1680. [Derham, p. 49.]

At about 10¾ hour in the morning, in *Gresham College*, I observed the falling of a great shower of hail; concerning which, I observed these particulars.

The day before, it rain'd almost, all the day, a gentle rain, and, by turns, the fore-part of the night. At about three or four o' the clock in the morning, was very much thunder and lightning, with an exceeding violent shower of rain; whether any hail then fell, I know not, being in bed; but, by some circumstances, I believe there did, for there were found, in the morning, several great spots of wet, which, 'tis probable, proceeded from hail-stones that fell down the chimney. It continued to rain, and now and then to thunder much, till about nine; then it clear'd up, and the sun shone very clear, and there was scarce a cloud to be seen; about ten it began to thicken, and I heard the thunder to the south east; at about half an hour after ten, it grew very dark, and thunder'd very near; and soon after there began to fall a good quantity of hail-stones, some of the bigness of pistol bullets, others as big as pullets eggs, and some above 2½ inches, and near three inches over the broad way; the smaller were pretty round, and white, like chalk, or sugar plums; the other of other shapes: some of the most remarkable were these.

Breaking many of them, I found them to be made up of orbs

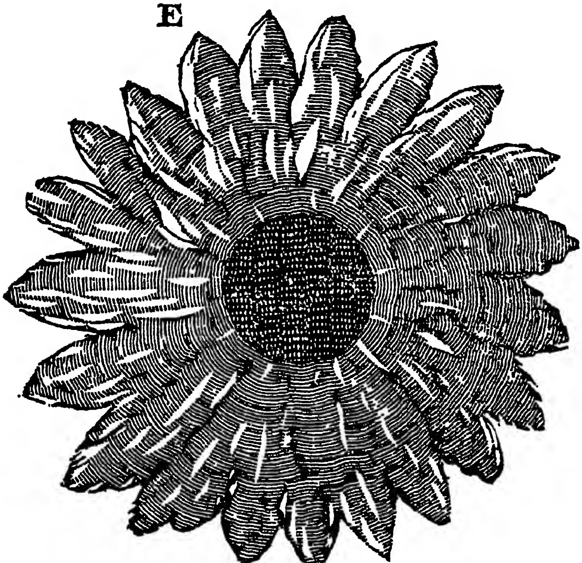
of ice, one encompassing another; some of them transparent, and some white, and opaque; some of these were to the bigness of near an inch in diameter, and were orbicular every way. Some of them had the white spot in the middle, as A; others towards



one side, as B; and the variety of white and transparent spots very differing; those, which exceeded these in bigness, were made by an additional accretion of transparent icicles, radiating every way from the surface of the white ball, like the shooting of niter, or toothed Sparre. These in some stood, as it were, separate in distinct icicles, which were very clear and transparent, and had no blebs or whiteness in them. Others of them were all concreted into a solid lump, and the interstices filled up with ice, which was not so clear as the *Stiriæ*, but whiter; and thereby one side, which,

I suppose, was the undermost, was flat, almost like a turnep; and the radiations appeared to proceed from the ball in the middle, more towards the upper side, and most toward the sides; the

E



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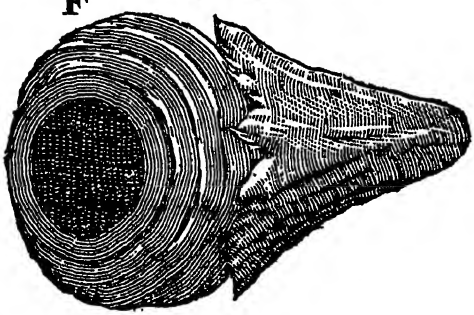


edges and top were more rough, and the ends of the *Striæ* appeared prominent; which the figures will better express.

The extent of this shower I cannot yet certainly learn, but have, by the information of several, understood it was seen above ten miles off. I was also told by several persons, that, a little before the hail fell, there was heard a great noise out of the

sky, like the shooting, or emptying, of a cart-load of pebbles, as if they had fallen one upon another in the air.

From the manner of their figure, I conceive, their accretion was made by a congelation of the water, as they fell; that the small white globule in the middle, about the bigness of a pea, was the first drop that concreted into hail; this, in falling through the clouds beneath, congealed the water thereof into several coats or orbs, till some of them came to the bigness of pigeons eggs, some white, some transparent, according to the several degrees of coldness it passed through, whilst they congealed; that the last accretion was made by a more violent and sudden cold, in the lower part of the cloud, where they passed through almost a continued body of water. Other varieties of their forms, which were very many, I conceive, must be made by their meeting with one another in their passage.[1]

Leichner's MSS.

May 27. Mr. Hooke produced a letter to himself from Eccardus Leichnerus, dated at Erfort, May 8, 1680, mentioning somewhat of a design, which he had, of sending over some writings of his, to be left with the society. But upon discoursing of the matter, and no person present being able to give a character of this author or of his writings, they demurred upon giving their positive answer till by a further information they could learn, whether they might deserve to be printed; or at least, whether it were convenient for the society to be anyways concerned for the publishing or not publishing of them.

Snake-stone.

Mr. Hooke mentioned, that he had found the place in Monsieur Tavernier's *Voyage*, where he speaks of a kind of snake-stone; and said, that he had there described the snake by a picture; and that the way of using it was by rubbing it against another stone; by which means a kind of oil was produced, which was made use of for the antidote against poisons.

[1] Notwithstanding Mr. *Waller* hath published the substance of this Paper, in Dr. *Hook's* Life, p. 22, yet the original may not be unacceptable to the reader, by reason of the figures, which the Doctor hath given of those monstrous hail-stones; which I, my self, saw falling, in great numbers, in *Great Lincoln's-Inn-Fields*, and notic'd to have fallen on *May* 19, 1680, one of which a servant brought me in his hand, as large as a turnep, and of the same shape, which I instantly measured with a string, and found the compass of the widest part to be above thirteen inches; which, I confess, seems somewhat incredible; but, I think, I did it with great care, and was not mistaken. W. Derham.

Hot Flame from Spirit of Wine useful for melting Glass.

Mr. Hooke showed an experiment, found out by His Highness Prince Rupert; which was, that a quantity of rectified spirit of wine being put into an aeolipile, and by the heat of a chafing-dish of coals converted into vapours, which issued out of the small hole of the aeolipile in a stream with great violence, would by the flame of a candle held under that stream be all fired and turned into a great and hot flame, which readily melted lead, glass, &c. and so might be of very good use for blowing the flame of a lamp for working glasses, as His Highness had at first designed it.

Mr. Hooke alleged, that he had made use of this way for driving out the air of small round glasses, in order to make several experiments with them; of which an account was extant in the Register for the year 1662; and that, if the society pleased, they might see the experiment of it upon this aeolipile: but it being late, this was referred to some other meeting.

Boyle's Experiments. Papin's Engine.

June 3. Mr. Hooke gave some account of the new experiments of Mr. Boyle, mentioned in his *Experimentorum novorum physico-mathematicorum continuatio*: as also of Dr. Papin's engine for boiling.

Letters from Nazzari and Leewenhoeck.

June 10. Mr. Hooke read a letter to himself from Signor Nazzari, dated at Rome, March 20, 1680.

He produced two letters from Mr. Leewenhoeck, which not being yet translated into English, were referred to the next meeting.

Aeolipile Experiment and ——.

Mr. Hooke gave an account of the experiments tried with the aeolipile; as also of what he, together with Dr. Tyson, had observed of * * * at the Lord Mayor's on the Monday before, while it was yet alive, and while it was cut to pieces by the cook. It was hereupon desired, that some care might be taken to procure some other hereafter, either from the Lord Mayor, to whom any strange fishes caught in the Thames are generally brought, or by some other means, that so they might be dissected and described more particularly than they had hitherto been.

Metals.

Mr. Hooke was desired to bring in his account of the trials about metals at the next meeting, and to think of some other subject to be prosecuted for the future.

Leewenhoeck's Letter.

June 17. A letter from Mr. LEEWENHOECK was read.

Organisms in Pepper-water.

July 8. Mr. HOOKE related, that upon long keeping of pepper-water, and renewing the water several times, as it had evaporated and dried away, he had observed a sort of exceedingly small animals swimming in it, which were perfectly shaped like fishes, such as minnows, or the like, in which he could plainly see the head and belly or bowels more opaque, and the tail or smallest part transparent.

Capillary Attraction.

After several experiments about the rising of liquors in small pipes had been tried, Mr. HOOKE explained his thoughts of it, and endeavoured to show the reason thereof from the congruity of the fluid bodies with the solid: but it was desired, that some further experiments should clear it.

Vibration of Glass.

After this discourse was ended Mr. HENSHAW read the second section of Century 1 of Lord BACON'S *Natural History of motion of bodies upon Pressure*: whereupon Mr. HOOKE related that he had observed that the motion of the glass was vibrative perpendicular to the surface of the glass, and that the circular figure changed into an oval one way, and the reciprocation presently changed it into an oval the other way; which he discovered by the motion of undulation or rising of the water in the glass which he discerned to be in four places of the surface in a square posture. It was moved that the experiment should now be tried before the Society, which was accordingly done; but that figure was not so plain, but that some doubted it.

Letter to Leibnitz.

July 15. Mr. HOOKE'S letter to M. LEIBNITZ concerning the farther usefulness of the philosophical language and character was read.

Aug. 9.

LETTER FROM HOOKE TO MR. OCTAVIAN PULLEIN AT ROME.

I received your most obliging letter of ye 15th of May last, and communicated the contents of it to the R. S., who were very well pleased with it, and ordered Dr. Gale, who hath undertaken that promise by reason I have not time to do it, to write to you, in order to hold a further correspondence with you. We much lament the loss of Signor Borelli, but are very glad to hear that his works will be published and return you many thanks for ye great favour in sending those specimina of it, which you inclosed.

It was very good entertainment, and caused much discourse about that subject, and Dr. Croon who pretends to the first motion of that hypothesis of muscular motion hath since brought into the Society the summe of what he some years since read at the Chirurgeons theatre on the subject. The pieces of Borellis sure will sell well, but especially his last. I have 4 of them, viz. *De medicis?*, *De vi percussionis*, *De mortinus naturalibus a gravitate pendentibus*, and the first edition of his *Euclid*, and should be very glad to have all his other works that are published. We long much to see the catalogue you are making, and should be glad to see the observations about Vesuvius, you speak of from Napells. I have not heard of Sign. Bocconi; his present will be acceptable to the Society as I doubt not his History of Corsica will be to all the world. The letter of Signor Nazzari I received and Dr. Gale has answered it, but I hear nothing yet of Signor Champigni. Mr. Martin died 3 of July. The new treatise about the river Tiber will be very acceptable, as also Sign. Fabretti treatise of Roman Aqueducts; as also Bellori's and Cav. Pozzo cuts and illustrations. I doe not find any body much inquisitive for ye collection of Coynes you mention. The catalogue of Mr. Boyle's works, I have sent you, but Mr. Martin died ye 3rd of this month, and so I knowe not how to prosecute the other. I have purchased (?) Mr. Hubery's litter. I should be glad to know whether there be any addition to Sr. Catalogue. Here are lately printed Mr. Boyle's New experiments of Compression and Dilatation of the air in which are many curious observations. And Dr. Tison's Anatomy of a Porpesse: [So far in Hooke's autograph. The copy continues in the hand of a less literate scribe.] Heare is one Warner an opothecary in St. Lawrance Lane who as found a poder by wich he does strangely preserve deed bodyes without taking out the bowells or mangling any part of the body we shall this week see a full Expt: of it. Here has bene lately a Frence[hman] who has been in the Indies and as made many curious observations which he Desiring shortly to publish, so brought with him into France a book curiously written in the Malabaric tongue upon the barkes of trees with many pictures in it, by which it is supposed to be the Bible in that language. But there is neither here nor at Parris anyone that understands it. He carried with him a painter who hath drawn all things that were curious that he [met] with as particularly the Ruins and inscriptions and Basso relives of Chilmenr, supposed to be Persepolis. Here is one, Mr. Piggot of Oxford, who is printing a very curious discourse about music which will have many considerable descoursings on that subject from many new experiments. Mr. Street who formerly published the Astronomie, is printing of new tables

whereby he pretends much to rectify all former tables and to make them for daily use that none hitherto extent can be anyways comparable to them. Heare are severall undertakens about making a perfect survey of England some about regulating the maps as Mr. Morgan one way, and Mr. Collers another, and Dr. Plot has allmost finished his Natural History of Staffordshire after the same method that he wrote his Oxfordshire. It will be published betwixt this and Xmas. One Mr. Mellon hath found a way of making glasses for microscopes so small that one of them which was double convex and not a perfect sphere weighed but fower hundredth part of a graine and yet was well polished as I was informed by one who saw it. [R. H.]

Sent Aug. 9, 1680. [MS. Sloane 1039, f. 170.]

Aug. 10.

Letter from HOOKE *to* M. JUSTEL.

Sr. I received the favour of your letter of ye [ ], but it was 3 weeks after its date before I received it, I would therefore beg yt what command you shall honour me with in for the future you would please to send it by the post, directing it thus, 'For Mr. Robert Hooke, Secretary of ye Royall Society at Gresham Colledge, London'. And I shall indeavour to return you a speedy answer. Mr. Chardin, whom you were pleased to inform me is now in England, and we hope to see somewhat of his curious observations about Persepolis. When he comes into France you will highly obleige the publique if you will persuade him to print it.

I have lately spoken with an English merchant who lived some time in Mosco who assures me that there is a trade from China to Mosco over Tartary by Tartars who bring severall sorts of china commodity's as wrought silks, china dishes, cabinets etc. of which he has seen some. We have also here a person who has lived 9 or 10 years in the Verone and can speak the Moscoitish and some Tartarian languages who would have been employed to have gone overland from Nova Zembla to China by land, to have found out whether there were any passage between those places by sea, designing to have kept all the way along the shore of it, if such there should be as our maps would make us believe, but he hath not been yet employed, he [has] given us a very good account of all the country of the Verana and of the manners and customs of the people. I should be glad to receive from yourself an account of what you may have seen of the phosphorus you mention, whether the person can, as he pretends, enlighten a whole room by it. I saw that of Mr. Craft which would kindle warmed gunpowder and paper, but the light it gave was but faint and it had no sensible heat here in one who makes the Phosphorus of Mr. Baldwin very well, but that is only of the

nature of the Bononia stone, and only returns what it receives from a light, but gives none of its own, however tis a very curious and truly luciferous experiment, and is of much use for finding the true nature of light. Whether Mr. Oldenburgh communicated all that he received from Mr. Baldwin to the Society I know not, but he may be assured that what he sends to me to that end shall be so impa^r^ted and I can assure him that the society will much esteem his correspondence, and he shall have suitable returns. The Smoking liquor of Mr. Boyle I have seen and tryd very often and know the composition thereof, but I never found that it had any light of its own, but it is of use to show the continual evaporation of some liquors into the air by the opacousness of the fumes which in most other such evaporating liquors are transparent and soe cannot be seen.

We should be very glad to know his way of printing without a press by a small and cheap machine. It would be a thing of very great use to the Publique, and especially to the Philosophicall World, especially if he could find an easy way to set and print off some few coppys as 20 or 30 of any discourse, which would not be much more troublesome or chargeable than the writing of 2 or 3 with a pen. Somewhat of that kind hath been here pretended to, and Sir Christopher Wren about 14 years since shewed an experiment how to doe it in one way, but I suppose it may be much facilitated, yet I should be very gladd to understand from you the new way invented for Removing Earth made use of in the King's building of which you were pleased formerly to promise me a description.

I know not whether you before this have seen the treatise which Mr. Boyle hath published about the Experiments made with condensed, expanded and factitious air. However I have taken care to have one sent you, as also another small tract of Dr. Tison of ye anatomy of a Porpesse. Here is one Dr. Wood who hath found out a new way of adjusting the account of time by the moon, soe as not to miss one day in 24000 years. It will shortly be published. Here is also one Mr. Johnson who is published a pocket herball, which I last night saw: it will contein 500 copper plates, and 5000 plants, and will have all the necessary descriptions judged best on one side of each leafe and the figure on the other.

Some days since I saw an engine for quenching of fire made by S[r]. Samuel Morland, in immitation of that of a German, which was here about 2 years since. The German's was the same, with that of Ctesibius by the compression of the air which gave a constant stream of water, but he used also leather pipes by which he conveyed the water from his engine to any convenient place for quenching the fire. This of Sir S. Morland is as he

pretends by other springs, and without making use of the air at all.

There is one Mr. Warren who hath a powder by which he pretends to preserve humane as well as other animall bodys from corruption for a very long while, which he doeth without opening or any ways disfiguring the body, and without loosing the colour. One Mr. Melling has found a way of making lenses for Microscopes soe small that one of them which is good weigheth not above $\frac{1}{460}$ part of a graine, and yet is a double convex, not a sphere. A gentleman hath lately found in Ginger water an infinite of exceeding small animalls, somewhat like those of pepper water, but smaller. A gentleman here acquaints me that he saw at Rome a mountebank that had an artificiall stone in a ring, that being wet would presently flame: if you know anything concerning it I should be glad to know it. [R. H.]

Translated into french by M. Pappin, and sent to M. Justel. Aug. 10, 1680. [MS. Sloane 1039, f. 174.]

Michaelmas Term.

Lectures on Light continued. [*Posthumous Works*, pp. 83–95.]

Roemer's Astronomical Instrument.

Nov. 4. Mr. Hooke produced a letter to himself from M. Justel, dated at Paris, August 21, 1680, N. S., giving an account of an astronomical instrument invented by Monsieur Roemer for finding the positions of the heavens at all times, either past, present, or to come, performed by clockwork within to be turned with the hand: as also such an instrument invented by Cassini, but without wheelwork, and to be set by the help of tables.

Boyle's Secret Letter.

Mr. Hooke showed a letter of Mr. Boyle, sent to him during the late recess of the society, wherein was contained some secret, which the author desired might not be opened but by his own permission.

Leewenhoeck's Letter.

Nov. 11. Mr. Hooke produced a long letter lately received by him from Mr. Leewenhoeck. It was written in Low Dutch; but the contents of the several heads were read by Mr. Hooke. It was desired, that the said letter should be translated and answered.

Water-level.

Nov. 25. Mr. Hooke introduced Mr. Meester, who explained his water-level; a print and description whereof were presented by Mr. Hooke for the use of the society.

Pulleyn's Letter.

Mr. HOOKE produced and read a letter to himself from Mr. PULLEYN, dated at Padua in the month of November.

Jupiter's Satellite.

Mr. HOOKE gave an account of the particular observations of the innermost satellite of JUPITER, recommended to him by Monsieur TOINARD, to be observed by the English astronomers, who might correspond with the French astronomers; together with an account of several observations made by Mr. FLAMSTEAD at Greenwich; which he designed to send to Paris.

Comet.

Mr. HOOKE gave a particular relation concerning his observations made on the Monday morning and Tuesday morning before of a new comet appearing in the beginning of the sign Scorpio a little to the southward of the ecliptic: That its motion was eastward, and hasted towards the sun: and that it would not be seen either on Wednesday or Thursday morning because of the thickness of the air.[1]

Nov. 29. HOOKE wrote to HEVELIUS on the Weather-clock. [R. S. MS. 56.]

Presidency of Royal Society.

Nov. 30. Mr. BOYLE was chosen President; but declining by a letter to Mr. HOOKE, dated Pall Mall, December 18, 1680, to accept of that office, and desiring the society to proceed to a new election of a President, Sir CHRISTOPHER WREN was elected into that post, into which he was sworn at the council held January 12, 1680/1.

Letter to Justel.

Dec. 2. Mr. HOOKE read a letter which he had sent by Mr. HALLEY to Mons. JUSTEL at Paris.

Boyle's Aerial Noctiluca.

Dec. 9. Mr. HOOKE brought in a book published and presented by Mr. BOYLE, entitled, *The Aerial Noctiluca: or, some new phenomena, and a process of a factitious, self-shining substance.*

Peruvian Bark.

Dec. 16. Mr. HOOKE read a letter to himself from Mr. NEWTON, dated at Cambridge, December 3, 1680, giving an account, that DOMINICO GASPARINI, doctor of physic of Lucca in Italy, had lately written a treatise of the method of administering the

[1] See Mr. HOOKE's *Posthumous Works*, p. 153.

Cortex Peruvianus in fevers, in which he particularly discussed, whether it might be administered in malignant fevers; and also whether in any fevers before the fourteenth day of sickness; and that upon the fame of the Royal Society spread everywhere abroad, he was ambitious to submit his discourse to so great and authentic a judgement as that of the society; and thereupon desired another doctor of physic of his acquaintance in Italy to write to his correspondent an Italian in London, to move, that the society would give him leave to dedicate his book to them. The said Italian being gone from London to Cambridge before the arrival of the letters, on the receipt of them applied himself to Mr. NEWTON, who promised him, that he would desire Mr. HOOKE to acquaint the society with Dr. GASPARINI's request. Mr. NEWTON added in this letter his thanks to Mr. HOOKE for the trials, which the latter had made of an experiment suggested by the former about falling bodies.

Mr. HOOKE was desired to answer Mr. NEWTON's letter, which he did in one dated December 18, 1680, in which he took notice, that the society was pleased with the subject of Dr. GASPARINI's book, the *Cortex Peruvianus* being a specific, which had of late been much discoursed of, and concerning which an ingenious physician of London, Dr. GOODALL, was then publishing a discourse, wherein he would endeavour to give a true account of the production and use of the *Cortex*, so far as he could be informed from writers, travellers, or his own experience. That as to Dr. GASPARINI's dedication of his book to the society, he needed no leave, things of that nature being usually done without asking a consent; but that doubtless the society could not but be very well pleased with the testimonies of respect from learned and ingenious persons, of which nature this was supposed to be: And therefore though they did not invite or prompt any persons to such addresses, yet the author needed not to doubt of finding such an acceptance and resentment thereof by the society, as might answer his expectations.

Cluver's Ephemerides.

Mr. HOOKE produced the first sheets of Mr. DETHLERUS CLUVERUS's *Ephemerides* presented by Mr. HAAK, containing a summary of his whole design in that undertaking; which being read was approved of.

Comet.

Dr. GALE was desired to request Dr. WALLIS to communicate such further observations on the comet, as should be made at Oxford.

Hereupon a discourse was held concerning the comet, and

Mr. HOOKE related what observations he had made concerning it, and what he conjectured concerning its place, motion, duration, &c., none of which could be positively determined till some further observations had informed him more particularly.

1680/1

Jan. and *Feb.* HOOKE delivered *Lectures on Light.*
[*Posthumous Works*, pp. 96–106.]

Pascall on the Earthquake and the Comet.

Jan. 12. A letter to Mr. HOOKE from Mr. PASCALL, dated at Chedzoy in Somersetshire, January 4, 1680/1, was read, giving an account of an earthquake, which had happened the day before at Chedzoy and other contiguous places; and of the comet now appearing.

Leibnitz.

Mr. HOOKE read a letter to himself from Monsieur LEIBNITZ, containing several ingenious conjectures, about the use of a universal language and character; as also of a universal algebra, and the great benefit thereof; and that by the help thereof he had been able to perform very many considerable things, which the commonly known algebra would no way enable him to do, &c.

Mr. HOOKE was desired to continue his correspondence with Monsieur LEIBNITZ, and to endeavour to prevail with him so far, as to procure from him some instance or experiment of these his new problems.

Justel's Letter.

Mr. HOOKE produced also a letter to himself from Monsieur JUSTEL, dated at Paris, January 1, 1681, N. S., wherein he gives the reason, why he could not yet send the description of a small printing engine newly invented; nor the account of the journey from Moscow to China; but that Monsieur THEVENOT had translated it from the Russian language into French, and was now printing it: That Monsieur PICART was printing an account of his voyages, and the observations made by him at Copenhagen, Montpellier, Bordeaux and Brest; to which he would add his treatise of the measures of the earth: That Monsieur ROEMER was returning to Denmark, being recalled thither: That Monsieur MARIOTTE had in the press a treatise on colours: That the comet had been observed but three times at Paris; and that the astronomers there judged it to be 62 degrees long, and to move two degrees a day: That it appeared small, and that the cloudy weather prevented it from being observed: That CASSINI affirmed, that just such a

comet had appeared three hundred years before: That Monsieur DU VERNAY had not yet shown his friends the observations, which he had made on the dissection of the fishes, which he had met with on the coast of Bordeaux; but that he had not seen any whale: That the drawings of what had been dissected were made by Monsieur DE LA HIRE with the utmost exactness: That there would be an account of the comet in an almanac printing in Paris, which contained several curious things, and particularly the longitudes and latitudes of places, as corrected by late observations: That Monsieur CHAMARS was busy in making experiments on the magnet: That KUNKELL, who invented the liquid phosphorus, had left Saxony, and was gone for Poland.

Taliput Leaf.

Mr. HOOKE showed a piece of a taliput leaf, which one Mr. KNOX, who had been nineteen years and a half captive in Ceylon, had brought with him from thence. It was about seven feet long and nine feet wide at one end, shaped like a woman's fan, closing and opening like that. The whole leaf was said to be a circle of twenty feet diameter.

Halley's Letter.

LETTER FROM HALLEY TO HOOKE.

Jan. 15. Halley wrote the following letter to Hooke from Paris.

"I got hither the 24th of last month, after the most unpleasant journey that you can imagine, having been forty hours between Dover and Calais, with wind enough.

"The letter you were pleased to intrust me with, did me the kindness to introduce me into the acquaintance of MM. Justel and Toynard, with whom is the rendezvous of all curious and philosophical matters. The general talk of the virtuosi here is about the Comet, which now appears, but the cloudy weather has permitted to be but seldom observed: whatsoever shall be made publick about him here I shall take care to send you. Whilst I am here I shall be able to serve you in procuring you what books you shall desire that are to be purchased for money; but those that have been published by the Academy of Sciences, amongst which is the book of plants Sir John Hoskyns desires, will be much more difficult to come by. However, I have hopes to get them for the Society's Library, at least, to get a sight of them, so as to give you some account of what they contain. There is just now finished the book of Astronomical Voyages, but I have not gotten a sight thereof. But Mr. Cassini, who seems my friend, will, I hope, grant it me. If I can but get it in my own possession, I will make hard shift to copy the most material things."

[Read to the R. S. on June 1. Weld.]

Magnetic Attraction.

Jan. 19. Mr. HOOKE showed his instrument for making experiments in order to find out the attractive power of the loadstone at several distances, and to reduce that power to a certain theory.

Adams's Survey of England.

Jan. 26. Mr. JOHN ADAMS of the Inner Temple was introduced by Dr. GREW; his business being to desire the advice and directions of the society in an undertaking, which he was engaged in, a perfect survey of England. He said, that he had already had the advice of the President, Mr. HOOKE and Mr. FLAMSTEAD concerning it.

Malpighi's Engraving.

Feb. 2. Mr. HOOKE gave an account, that he had spoken with Mr. LOGGAN concerning the engraving of Signor MALPIGHI's picture; and that he had seen it, and was willing to undertake it.

Gnats.

Mr. ASTON returned *Sangallo della Zanzani*, and gave an account, that there was very little in the book new, but that the author's design was only to show, that gnats were not produced of the mud of water, but from eggs: that most of the observations were the same with those published by Mr. HOOKE in his *Micrographia* and by SWAMMERDAM in his book on insects.

Present of Books for Malpighi.

Mr. HOOKE was desired to think of some fit books to send to Signor MALPIGHI for a present from the society.

High Barometer and Parhelia.

Feb. 23. Mr. HOOKE acquainted the society with his observation on the height of the mercury on the 7th of January preceding, when, according to a paper, which he showed, the three suns and several rainbows appeared in Sweden. Mr. HOOKE's observation was, that the mercury at that instant stood higher than ever he had remarked it to do before, though he had constantly taken notice of it for near twenty years: that accordingly he had caused Mr. HUNT at that time to take notice of it, and measured it, though he knew nothing of the phenomenon in Sweden till the last week.

Papin's Engine.

Mr. HOOKE presented to the society Dr. PAPIN's engine for boiling bones, &c. which the doctor had left for the use of the society. It was opened, and all the several parts of it explained

by Mr. HOOKE, together with the method of fitting and using it for boiling, &c.

It was desired, that some trials should be made with this engine at the next meeting.

Mar. 1.

LETTER FROM HOOKE TO M. TOYNARD.

Sr., I received a letter from Mr. Lock (whom your letter found at Oxford) desiring me to send you my Discourse about Levells, but the thing being not yet printed, I was not able to comply with his desire, but soe soon as printed I shall take care to transmit one to you. I have not since received any more observations of the Satellites from my astronomical correspondents, but soe soon as I doe you shall have an account of them. We long very much to hear what the gentlemen of the Royal Academy have done on that subject, as also what other astronomical observations they have published. I have here sent you my Animadversions on Hevelius his Machina coelestis, because there is somewhat about levells in that discourse. I am, Sr, your most humble servant. [R. H.]

[With a Latin translation on the back.]

March 1, 1680. [MS. Sloane 1039, f. 175.]

Standard Thermometers.

Mar. 2. Mr. HOOKE gave an account of the method of adjusting thermometers by a standard, according to which all that were made by Mr. RICHARD SHORTGRAVE were adjusted; a short account of which Mr. HOOKE had formerly published in his *Micrographia.*

Papin's Engine.

Part of a letter of Dr. PAPIN to Dr. CROUNE dated at Antwerp, March 1, 1680/81, N. S., was read, wherein he mentioned his having, before his departure from London, left at Mr. HOOKE's lodgings his engine for softening of bones, &c. to be presented to the society; and his desire and readiness to serve the society in the places, where he should reside.

An experiment was made in Dr. PAPIN's engine, wherein were put pieces of ivory, horn, and tortoise-shell; all which were in about the space of half an hour reduced to softness; the tortoise-shell to the softness and pliableness of shoe-leather or tanned leather, the ivory to the consistence of old Cheddar cheese, and the horn to the softness and pliableness of pretty stiff tanned leather.

Erkar on Mines *and a German on the Comet.*

Mar. 9. Mr. HOOKE was desired to peruse and help in the

translation of the book of LAZARUS ERKAR on *Mines*. He also delivered in a German book sent by Sir PETER WYCH from Hamburg, being an account of the appearances of the comet before and after its conjunction with the sun.

? *March* 16.

LETTER FROM HOOKE TO R. WOOD.

Sr. I last night received your present very safe and shall take care to present it to the R. S. this day at their meeting when I doubt not but that it will be extremely acceptable, not only as it is a jewell in itself but more especially as it is a testimony of your respect, which I am sure is highly valued by them. The receipt I shall be very careful of returning to you very safely. If you please to direct me to any one to whom I may intrust the conveyance of it to you, I shall observe your commands, I cannot meet with a person I know fit for a tutor, but if you please, I will send to a mathematicall friend at Oxford who is acquainted with all versed in that sort of learning in the University to get his assistance, if you please to let me know what you desire from him, and what he may expect. I am very sorry I came not in time enough by the post last night to give you this short advertisement from

Sr,

Your most humble
and most obedient servt.

R. H.

[MS. Sloane 1039, f. 176.]

Dr. Wood's Almanac.

Mar. 16. Mr. HOOKE presented from ROBERT WOOD, LL.D. one of his almanacs put into a gilt frame for the use of the society, and another to the President.

Mercator's Projection.

The President acquainted the society, that Mr. MERCATOR had lately shown the King a new way or projection of maps useful for seamen; but did not mention the particular method of it.

Mr. HOOKE said, that Mr. MERCATOR had been with him to discover to him his projection; but that he was not willing to understand it from him, in order that when his own, which he already discovered to the President, should come out, it might not be thought, that he had taken any part of Mr. MERCATOR'S invention.

Voigt's Cometa.

Mr. HOOKE produced a paper, which he had procured from Mr. CLUVERUS, who had taken the pains to peruse JOHN HEND-

RICHT VOIGT's book called, *Cometa matutinus et vespertinus*, and had made an extract of the same; which was read.

Chelsea College.

Mar. 23. Mr. HOOKE was present at a meeting of the Council and was desired to discourse with Mr. CHENEY as to the ground at Chelsea College.

Mercator's Projection.

Mar. 23. Sir CHRISTOPHER WREN now somewhat explained Mr. MERCATOR's new projection for maps for the use of the seamen, viz. that it was a certain projection of the surface of the globe upon a plane, parallel to the plane of the equator: That the pole was the centre; and the parallels concentric circles; the meridians straight lines passing through the pole or centre; all which are common with several other polar projections already known and used: But the singularity of this was, that the distances of the parallel circles were proportioned according to the proper spreading of the meridians somewhat of the nature of the chart of Mr. WRIGHT (commonly, but falsely, called MERCATOR's chart) to which he had a certain scale appropriated so, as that he could easily measure distances.

1681

On Light.

Apr. 20. Mr. HOOKE read a long discourse about the nature of light and luminous bodies.[1]

The experiment of fire burning in a box was exhibited and explained; and it was shown how it was pertinent for the explanation of the theory of light in the above-mentioned discourse of Mr. HOOKE.

Luminosity.

Apr. 27. Mr. HOOKE read another discourse concerning the nature, cause, and effect of light and luminous bodies, wherein was an enumeration of all such bodies, as afford light, and the manner how they might be made luminous.[2]

It was desired, that the experiments for the next meeting might be about the explication of light.

Adams's Survey of England.

May 4. Mr. JOHN ADAMS presented his proposals for making an actual survey of England by measuring the bounding line,

[1] See his life by Mr. WALLER, p. 23; and his *Posthumous Works*, p. 71, et seq.

[2] See his *Posthumous Works*, p. 71, et seq.

the distances between places both in the road and the straight lines, by taking the latitudes and angles of position; desiring the directions and encouragement of the society in this undertaking.

It was referred to Sir JOHN HOSKYNS, Dr. PELL, Mr. HILL, and Mr. HOOKE, to consider of the said proposals against the next meeting.

Theory of Light.

Mr. HOOKE read another discourse[1] about his theory of light.

An Arresting Medium in vacuo.

Mr. HOOKE supposed the cause of the stay of a pendulum *in vacuo* to be from a body of a middle nature between ether and air, the motion, gravitation, and density of which he conceived also to be the cause of divers of the phenomena, which he should have occasion to discourse more of in his further inquiries about light.

This, he affirmed, would be likewise useful in the explication of the motions of the celestial bodies, as the moon and other planets.

Mercator's Projection.

Mr. HOOKE explained and demonstrated a certain property in the projection of the planisphere not taken notice of by any writer; which was to show the proportion between the substances of the arches of great circles in the globe and those lines projected in the planisphere to be the same; and how by that means the distances of any two places in a map so projected on the pole of the world, or the distance between any two stars in a projection of the heavens, either on the pole of the equinoctial or ecliptic, might be certainly measured by the help of a line of chords on a sector, which he supposed would do all that was pretended to be done by a new projection of Mr. MERCATOR, of which, he said, Mr. MERCATOR had not yet discovered the ground nor the demonstration, but alleged it to be upon another principle.

The President was satisfied with the explanation and demonstration of Mr. HOOKE; and desired, that he would bring in his demonstration in writing; as also his method of solving by it all spherical triangles, more easily than by the common ways.

Survey Committee.

May 18. It was ordered, that Mr. LANE be added to the committee appointed May 4, for sealing the writings; and Mr. HOOKE was desired to procure a meeting of Mr. LANE with Sir JOHN HOSKYNS immediately.

[1] *Posthumous Works*, p. 107.

Mercator's Projection.

Mr. HOOKE acquainted the society, that on the Saturday before he had met Mr. MERCATOR at Mr. BERRY's, where, after Mr. HOOKE had told him, that he had demonstrated the property of the planispherical projection for measuring distances of places in different longitudes and latitudes by the help of a line of chords on a sector, who would serve indifferently for all magnitudes of that projection; and had also shown him the manner of measuring thereby, which Mr. MERCATOR desired to see a second time; the latter very often and very positively affirmed, that his projection was not like that, but of another nature, and was no optical projection, but done by a certain proportion, which he had found, of dividing the meridian line, somewhat of the nature of Mr. WRIGHT's or MERCATOR's projection, as it was called, for the finding the rhomb and distance: that his projection was finite, and did not extend by the proportion of the half-tangent, but was finished with a circle not very far extended, which represented the south pole: with many other expressions of the like nature, by which he wholly disclaimed this projection. But that on the Monday after Mr. MERCATOR came to Mr. HOOKE's lodgings with Mr. AUBREY, where he positively said, that his projection was no other than that of the common planisphere; and that though he had made use of another way of measuring, yet he understood the way of using the sector. To which Mr. HOOKE answered, that though he was sufficiently assured of Mr. MERCATOR's ability, yet it did not evidently appear to him, that he, Mr. MERCATOR, had known either that property of the planisphere or the use of the sector in that particular, before he, Mr. HOOKE, had demonstrated it; or that the projection, which he had shown the King, was the same with the planisphere, since he had informed Sir CHRISTOPHER WREN as well as many others, that it was not that, nor any optical projection.

Mr. HOOKE dissuaded Mr. MERCATOR from taking out a patent for the said invention; since, as it could not restrain any person from making maps in that way, it having been so old, common, and practised a way of making maps; so, though it could, yet he assured Mr. MERCATOR, that he himself was making maps by another way, the properties of which far exceeded those of the planisphere; for which nevertheless he would not take the benefit of a patent, but desired, that the use and benefit thereof might be free.

Pendulums in vacuo. *Spring Suspension.*

A discourse was occasioned about the pendulums standing still in an artificial vacuum: whereupon some conceived, that it

was occasioned by the bending of the string; others from the weight of the string or wire: but Mr. HOOKE answered those objections, and demonstrated, that it could not be either the bending of the string, or the weight of any part thereof, that must cause that impediment. And upon that occasion he acquainted the society with a way of hanging a pendulum by a very broad and thick spring, which he had many years since invented and explained in one of his lectures in the year 1666. That he had since made divers clocks that way: and that he had acquainted Mr. TOMPION with it, who had also several times used it.

Mayerne on Staining Agates.

Mr. HOOKE read a translation, which he had made of a paper of Sir THEODORE MAYERNE, brought in by Sir THEODORE DE VAUX, about a method of staining agates.

May 25. The minutes of the preceding meeting were read; which gave occasion to discourse about the ways of staining agate mentioned in Sir THEODORE MAYERNE'S paper communicated by Sir THEODORE DE VAUX: and it was generally concluded, that the ways mentioned in that paper were only conjectures, and not the result of experiments; and that there were other ways of opening the bodies of stones, so as that they might imbibe colours, than by means of spirit of nitre, which would rather corrode a stone than sink into it. Thereupon Mr. HOOKE related, that he had been lately informed, that a goldsmith in Lombard Street had an agate by him, that had been so stained.

Letter from Halley.

June 1. Mr. HOOKE communicated a letter to himself from Mr. EDMUND HALLEY, dated at Saumur, May 19, 1681; containing some articles of literary intelligence, and particularly an account of the books published by the Royal Academy of Sciences, with a short account of Monsieur CASSINI'S hypothesis of the comet: upon which Mr. HOOKE showed Monsieur CASSINI'S book on that comet. [Cf. HALLEY'S letter of Jan. 15 ult.]

Mr. HOOKE was desired, when he should write next to Mr. HALLEY, to inquire what was become of the works of Monsieur MYDORGE, which were not printed.

Comet.

Mr. HOOKE showed a collection of observations of the comet made at Nuremberg, written by Dr. EIMMERT to Dr. ARNOLDUS,

who communicated them to the society; but there was not time enough to peruse them.

Mural Quadrant at Nuremberg.

Mr. HOOKE showed also a draft of a mural quadrant made use of at Nuremberg.

Cassini on Comets.

June 8. Mr. HOOKE gave a full account of Monsieur CASSINI's book concerning the late comets, together with some animadversions on that astronomer's theories for explaining the motions of them.

Comet of 1667.

He also showed a draft sent by Mr. SAMBROOKE of a comet, which was said to be seen in the East Indies in the year 1667, but not seen here, being far removed to the south: And he explained a theory of his about light, and the manner how the eye becomes the organ of sight, and seems immediately to feel the action of the luminous body, though ever so far distant, as if actually on the bottom of the eye.

June. HOOKE continued his *Lectures on Light.*[1]

Augmentation of Salary.

June 22. In consideration of the propositions made by Mr. HOOKE for a more sedulous prosecution of the experiments for the service of the society, and particularly the drawing up into treatises several excellent things, which he had formerly promised the world; the council as an encouragement, according to the small abilities of the society, agreed to add to his salary forty pounds for this year ending at Christmas Day.

Tanning Leather.

July 13. Mr. HOOKE mentioned the way used by the Indians in Virginia for tanning their leather by the help of fire, making use of the brains of the creature mixed with the oil of the hickory and oyster-shell lime.

Narborough's Voyage.

The society desired, that Mr. HOOKE would peruse the relation of Sir JOHN NARBOROUGH's Voyage through the Straits of Magellan to Baldivia; and that, if it contained anything very considerable, it might be transcribed.

[1] *Posthumous Works*, pp. 119–28.

New Helioscope.

Mr. Hooke showed a new helioscope, which he had lately made, whereby the figure and true shape of the sun, and the spots of it might be better observed than by any ways yet made use of. He explained the particulars of it.

Iris Diaphragm.

July 27. Mr. Hooke showed his new-contrived aperture for long telescopes, which would open and close just like the pupil of a man's eye, leaving a round hole in the middle of the glass of any size desired; which was well approved of.

Sounds from Cog-wheels.

He showed an experiment of making musical and other sounds by the help of teeth of brass wheels; which teeth were made of equal bigness for musical sounds, but of unequal for vocal sounds.

Helioscope.

He showed likewise his helioscope perfected by a double reflection, which would exhibit the figure of the sun very perfectly.

Sept. 15.

Letter from John Wilcox to Hooke.

[MS. *Sloane* 1039, f. 105.]

Extracts from Hooke's *Diary.* [MS. *Sloane* 1039.]

A few leaves of a Diary kept by Hooke on narrow slips of paper about 8 in. × 3 in., give a glimpse of his multifarious labours between the months of Oct. 1681 and Sept. 1683.

The wheel of unequal teeth: musick: the dipping needle poised.

[I want figures 48, 49, 50, 51 in Monconys voyages. *erased.*]

NB. Monconys Amalgam Burning glass.

M. An object glass by flexure with rings and screws.

NB. Rumb line found Oct. 8, 1681. Paid (?) Mr. Haak for Justell's letter 9th Oct. 1681.

Oct. 25, 1681. Demonstrated the foci of all sphericall glasse.

NB. The apparent number of 4 vanes moved swift. Instrument reflective.

[MS. *Sloane* 1039, f. 152.]

Natural History of Barbados.

Oct. 19. Mr. Hooke read a letter to Mr. Evelyn from Mr. William London, dated at Barbados, December 28, 1680, wherein he declared his intention of writing a history of Bar-

bados, and therein giving a true account of the situation, survey, seasons, natural productions, plantations, people, artificial curiosities, trade, government, governors civil, military, and ecclesiastical; of the customs and manners of the people, &c. designing also to do the same thing in the same method for all the other English plantations in America; and adding the heads of his discourse: which being likewise read, were judged very full and exact. But having in his letter desired the advice and assistance of Mr. EVELYN and of the Royal Society, a committee was appointed to consider further of his proposals; and to communicate to Mr. EVELYN what they should think proper to be further done by the said Mr. LONDON. The persons named of this committee were Mr. HILL, Mr. ASTON, Dr. PLOT, Sir JOHN HOSKYNS, if in town, and Mr. HOOKE; who were to meet the next day at Gresham College.

Nutmeg Trees in Holland.

Mr. LONDON having also requested Mr. EVELYN'S assistance in procuring him by some means some of the spice trees from the West Indies, the same was discoursed of, and the difficulty of such a business mentioned. However, Mr. EVELYN remarked, that VAN MUNTING had produced those trees, and kept them growing in Holland: and Mr. HOOKE related, that he had been lately informed by one Mr. WHISTON, a broker, that he had procured out of Holland three nutmeg trees growing in pots. It was notwithstanding looked upon as extremely difficult to procure any such for Mr. LONDON.

Justel on Quinquina and Loadstone.

Mr. HOOKE showed a letter of Monsieur JUSTEL, giving some account of the quinquina or Jesuit's bark; as also of the increasing of the power of the loadstone. Monsieur JUSTEL being now come to England, it was hoped, that he would give a further account of those and other particulars mentioned in the said letter.

Rhomb Lines on a Polar Projection.

Mr. HOOKE showed a new invention of his concerning the true figure of the rhomb lines in the polar projection of the planisphere, as also a way of drawing all the said lines true upon such a projection of any bigness; with a method of finding the length of any part of any such line; and of straightening the said line or any part thereof with ease and certainty, and thereby answering many questions in navigation without calculations by the help of a ruler and compasses without the use of tables.

Leewenhoeck's Letters.

Nov. 2. Mr. HOOKE produced a letter, which he had newly

received from Mr. LEEWENHOECK, dated at Delft, November 4, 1681, N. S., which being written in Low Dutch, and very long, was referred to the next meeting: and in the meantime Mr. HOOKE was desired to procure an English translation of that letter.

Dr. CROUNE moved, that Mr. LEEWENHOECK's letters might be all printed: to which Mr. HOOKE answered, that the greatest part of them were already printed in the *Philosophical Transactions* and *Collections*; and that there was another of them going to be printed; and that he designed to publish the rest in some succeeding *Collections*.

Method of Finding the Focus of a Lens.

Mr. HOOKE showed a mechanical way of finding the focus of all parallel rays falling upon the spherical superficies of a more dense refracting medium (whereby would be avoided the tediousness of calculating several triangles for the finding the focus of every single ray) by the motion of a certain circle upon a point in its diameter eccentrically taken, according to a proportion assigned.

He also showed the geometrical ground and demonstration of the same: and though it was denied by Mr. FLAMSTEAD as false and impossible, yet Mr. HOOKE persisted in his proposition, and referred it to be judged by Sir CHRISTOPHER WREN, who, he doubted not, would easily satisfy the society of the truth and evidence thereof.

Mr. HOOKE likewise engaged to produce at the next meeting a mechanical way of finding the foci of all the parallel rays falling perpendicularly upon the plane of a plano-convex glass, where the convexity was turned towards the foci thereof: as also the geometrical demonstration of the truth and certainty thereof; which Mr. FLAMSTEAD also denied as impossible.

Leewenhoeck on Micro-organisms in Diarrhœa.

Nov. 9. Mr. HOOKE produced a translation of the long letter lately received from Mr. LEEWENHOECK, and dated November 4, 1681, N. S., which he read; containing an account of divers observations and discoveries, which he had lately made concerning great numbers of small animals in his excrements, which were most numerous when he was troubled with a looseness, and very few or none, when he was well. He also found the same very observable in the excrements of other animals, &c.[1]

Demonstration of Foci of Plano-convex Lenses.

Mr. HOOKE showed and demonstrated a very expeditious way

[1] Printed in *Philosophical Collections*, iv, p. 93.

of finding all the possible foci of parallel rays refracted by a plano-spherical lens; whereof the convex side was turned towards the focus; as also what quantity of rays would pass through such a glass, whose convexity was the full bigness of a hemisphere.

Mr. Hooke likewise acquainted the society, that Mr. Flamstead had now acknowledged, that what he had formerly adjudged against the problem shown and demonstrated at the last meeting by Mr. Hooke, concerning the foci of parallel rays refracted by an hemispherical surface, was a mistake of his; and that upon considering it more seriously he had found out the demonstration, though he had not done it before the way of demonstrating it was shown by Mr. Hooke.

Geometry Books for Library.

Nov. 16. The President [Sir Christopher Wren] discoursing concerning the library of the society, promised to give the society five pounds to be expended in books of geometry: and Mr. Hooke was desired to find out such books, as he should find proper, and were not already in the library.

Upon a discourse concerning instruments useful for the sea, Mr. Hooke mentioned some contrived by himself, which would be of great use for taking azimuths, altitudes, &c. by a new way not before practised, which he designed shortly to publish. He observed likewise, that he had been newly informed by a person skilful in sea affairs, who had been several voyages to the East Indies, the Straits, and elsewhere, that he had been able to see the horizon at sea on a starlight night, and so to take an altitude from it: and that he could thereby find the variation of the compass.

The President was desirous, that a good and easy contrivance should be thought of, and made for an azimuth compass, in order that observations of that kind might be made more often and more certain by seamen; the difficulty of making which observations with the instruments now known being the greatest reason, why there were so few good observations of that kind. He further observed, that no good observations could be made at sea by the help of a perpendicular, which was the reason of discontinuing the use of the astrolabe, and making use of the sea-quadrant and back-staff.

Quadrant for Use at Sea.

Mr. Hooke mentioned a quadrant of his contrivance, which he was now making, and would shortly produce, and which, he conceived, would be much more accurate than any yet used

for that purpose, and which would obviate divers objections: but that some parts of it were yet unfinished.

Hair of Morse.

Mr. HOOKE showed the beard of a morse, which was very remarkable, having strong and crooked bristles much like horn, but harder and bigger than the teeth of a large horn-comb, or of a wheat-straw, and hollow at the roots in the flesh.

Refraction.

Mr. HOOKE brought in the demonstration of a new way of solving the phenomena of refraction, by supposing the denser body to refract towards the perpendicular; and shewed, that the line of light, that passes through two or more different media obliquely to the contiguous surface of the transparent media, the bulk of the bodies of both media lying in that refracted line, is the least quantity, that is possible to be moved to communicate motion from the luminous to the enlightened body.

New Compasses.

Mr. HOOKE also acquainted the society with two sorts of compasses, which he was making, for describing all sorts of spiral lines for the rhombs.

Projection of Globe.

He mentioned likewise, that Mr. FLAMSTEAD had now confessed, that he was mistaken in his assertions against the problem, which Mr. HOOKE had formerly demonstrated to the society concerning the new way of measuring distances in great circles laid down on the planisphere projection of the globe by the help of a sector and compasses, without drawing lines or circles, or making any divisions, &c. which had been by Mr. FLAMSTEAD before the society impugned as false; but now he acknowledged it to be true and real, as demonstrated by Mr. HOOKE.

Leewenhoeck's Lenses.

Nov. 23. Mr. HOOKE was of opinion, that the small magnifying lenses used by Mr. LEEWENHOECK were no other than those mentioned in the preface to his *Micrographia*, viz. very small transparent globules used whole, or by grinding reduced to a lens, or by another way, mentioned also by him in the same preface, much more easy to be made; a specimen of which, he said, he would shortly show, as he had long since done to the society. These he conceived were helped by the way of admitting light upon them in an appropriated room by the extraordinary

pains and care of Mr. LEEWENHOECK, in examining the objects in various ways.

New Instrument for describing Rhombs on Polar Projection.

Mr. HOOKE produced a new sort of instrument for describing the rhombs or spiral lines upon the plano-spherical projection on the pole of the world; and he showed how the same would easily describe all manner of proportional spirals, whether greater or less, whether wider or narrower; and mentioned also what use it might be of for navigation and sea-charts.

Orders of Council.

Dec. 7. It was ordered, that Mr. HOOKE should take care, that all orders of council should be transcribed into the Journal Book.

Sparks from Heated Iron.

Mr. HOOKE gave an account of a small treatise of JOHN DANIEL MAYER, printed at Sleswick in 1679, and entitled, *Consideratio ferri radiantis, quâ in naturam ignis aut lucidi spiritus utcunque inquiritur. Quaedam de thermis novo artificio parandis adduntur.* In which was contained an account of a very strange experiment to be made with a piece of iron heated till it be almost ready to melt; from which iron there will be emitted or darted forth every way a great number of sparkles, some of which will be of a most pure silver bright light. These the author affirms to be without any hurtful heat; so that being received on the back of one's hand they will not burn, nor affect it at all with heat. But Mr. HOOKE remarked, that he had newly tried the experiment with Mr. HUNT, and could not find any such effect; but on the contrary, that both of them had found the sparkles to burn or scale the back of their hands: and that, together with those bright sprinklings, there were other red-hot ones emitted, which, he said, MAYER affirmed, would burn; from whence he raised a new theory of light and fire. It was desired, that the experiment should be further tried, and an account thereof brought to the society.

Force of Gravity tested by Pendulum Clocks at the Monument.

Mr. HOOKE produced two pendulum clocks, which he had procured and adjusted, in order to make trial of a curious inquiry concerning the attractive power of the earth at several distances from its centre. This was done by placing one of them at the top, and the other at the bottom of the pillar on Fish Street Hill, and accurately examining, whether they would keep together; or whether that, which was nearest to the earth, would

go faster. At which trial Mr. HOOKE desired, that some other members of the society might be present to bear testimony of it.

Milk-white Glass.

Mr. HENSHAW produced a paper containing the milk-white pieces of glass, which had been so made by the corruption of a menstruum contained therein. This substance was very brittle, and with one's fingers might easily be crumbled into sand. This was received from Mr. JOHN DWIGHT of Fulham. [*Early Science in Oxford*, i, p. 27.]

Mr. HOOKE remarked, that he knew a menstruum, that would produce the same effect, viz. being put into a glass it would in a short time so corrode it, as to turn it all into a brittle milk-white substance, like that now produced before the society; but the parts of the glass above the surface of the menstruum would receive no damage by it.

Mirage.

Mr. HOOKE mentioned an observation which was affirmed by a minister of Sligo in Ireland, that he himself, and several hundreds more, had seen in the sea the perfect representation of an island rising out of it, as it were, with trees on the hills very plain and conspicuous; but as soon as the sun was set, it perfectly disappeared. This was related by the person himself, being an archdeacon, in the presence of Dr. WILLIAM LLOYD, Bishop of St. Asaph.

Dec. 10.

(30) *An Optical Discourse, proposing a way for helping short sighted or purblind eyes.*

Philosophical Collections, No. III, p. 59, 1681.

(31) *A Mechanical Discourse, containing a description of the best form of horizontal sayls for a Mill, and the ground of the inclined Sayls of Ships.*

Philosophical Collections, No. III, p. 61, 1681.

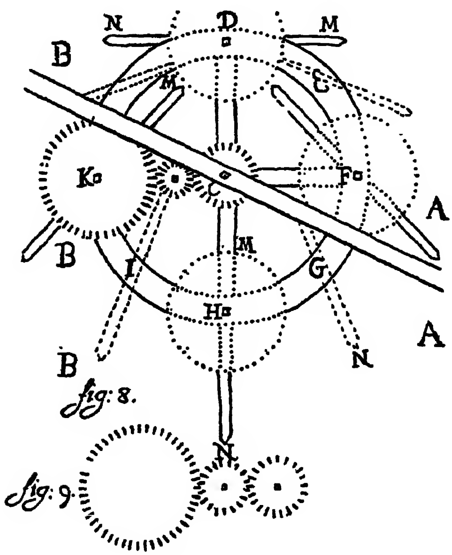


FIG. 8. WIND VANE.

A to B is the supposed direction of the wind. MN vanes set at appropriate angles at the ends of arms DH and FK, radiating from axial point C.

FIG. 9. Train of three cog-wheels for setting the vanes at the most advantageous angle to the wind.

Geometric Turning.

Dec. 14. Mr. HOOKE showed an engine for exactly describing all manner of proportion and helices upon cones and cylinders; as also of making or turning any variety notable in the shape of any fish shell; and all helices, screws, crenated, foliated, echinated, wreathed, &c. conchoids; and he observed, that this engine would be of great use for making the divisions of mathematical and astronomical instruments, for turning wreathed work, and many other uses.

Divers members of the society were well pleased with the contrivance; and Mr. PACKER urged very earnestly, that a complete engine should be forthwith made; at least, that some work-

men should be consulted what such an engine well made would cost: and he desired, that a description of it might be brought in by Mr. HOOKE.

Engine for Drawing Lines on Globes.

The experiment proposed by Mr. HOOKE for the next meeting was an engine to draw all the rhomb lines upon a globe as truly and exactly, as any greater or lesser circles could be drawn upon it; which had never been yet done by any person; with an explanation of the true nature and properties of all such lines, &c.

Freezing by Eggs and Apples.

Dec. 21. The mention of heat occasioned a discourse concerning the effects of cold, particularly the phenomena observed of frozen eggs and apples, that they being put into water would in a short time freeze about themselves a certain shell or crust of ice.

Mr. HENSHAW supposed this to be produced from the particles of cold, which issuing out of the egg or apple, and entering into the water, cause the parts of the water to freeze into ice, and so leave the parts of the egg or apple unfrozen.

Mr. HOOKE conceived it not to be by any really issuing of particles of cold, but that the egg or apple needing a much greater degree of cold to freeze them than the water, they being already frozen, and so having in them that degree of cold, and the water being yet unfrozen, and having a degree of heat in it enough to keep it unfrozen, they being put together, reduce each other to a middle degree of temper as to heat and cold, which is cold enough to freeze the water into ice, but yet warm enough to leave the egg or apple unfrozen.

Dr. GREW supposed, that it might proceed from the spirituous part contained in the egg or apple; and the water of it, in the water, in the same manner as spirit of wine would remain unfrozen, when water with the same degree of cold would be turned into ice.

But Mr. HOOKE supposed, that there was no need of having such a spirituousness as vinous spirits, because there are instances of other bodies, that require much greater degree of cold to freeze them than water, some of which were yet much less spirituous in that kind than water, as a strong brine or solution of salt, and quicksilver, which no cold had been yet found sufficient to congeal; and because there are other bodies more spirituous than water, which yet congeal sooner, as oil of aniseed, which grows hard with a much less degree of cold. He therefore supposed it to be from the particular texture of the body, which might be of a very different nature in other respects from a

vinous spirit, and yet might agree with it in this, of being subject to be frozen.

Engine for Drawing Lines on Globe.

Mr. HOOKE produced a globe of about a foot diameter so fitted with an instrument, that he could thereby both geometrically and mechanically draw all the rhomb lines upon it most exactly: which he explained, and by several experiments proved the truth thereof.

1681/2

De Fontaney on the Comet of 1680–1.

Jan. 4. Mr. HOOKE gave an account of a book, which he had newly procured from Paris, of Father DE FONTANEY, a Jesuit, containing his observations and hypothesis of the late comet of 1680 and 1681; together with some discourses about, and censures of the hypothesis of several other writers. This account being long, was reserved for another meeting.

Geometric and Dividing Engine.

Mr. HOOKE showed an engine for describing all manner of helices upon a cone, which he affirmed to be able to divide any given length, though exceedingly short, into almost any assignable number of given parts; as for instance, an inch into 100,000 equal parts, and that with the greatest ease and certainty imaginable; which he conceived to be the best way yet thought of in the world for perfecting all manner of astronomical and geographical instruments.

£40.

Jan. 11. In pursuance of an order of June 22, 1681, it was ordered, that the treasurer pay Mr. HOOKE the sum of £40.

Calculus of a Turtle.

Mr. HOOKE brought in a present from RALPH BOX, Esq., of a very large stone, or calculus of a turtle, formed like a bezoar stone.

New Instrument for Drawing an Inverted Parabola.

Mr. HOOKE showed a new instrument, by which he described a certain curve line, which might be called an inverted parabola, or parabolical hyperbola, having these properties, that it is infinite both ways, and hath two asymptotes, as an hyperbola; and that one of the asymptotes being laid upon the axis of a parabola, and upon which occasion lines drawn parallel to it, cutting the parabola and curve, the tangents of the curve are

always at right angles with the tangents of the curve. This gave occasion to much discourse concerning some properties of the same; as also, whether it would ever arrive at a certain line drawn upon the cylinder, &c. which Mr. HOOKE affirmed it would never do, though it approached nearer to it every revolution.

Moxa.

Jan. 18. Mr. HOOKE was of opinion, that there might be some peculiar virtue in the very substance of the moxa, which might cause this effect, besides the heat and cauterizing; which virtue might lie in the solid oil of it, which most vegetables upon burning yield, as tobacco, paper, linen, &c. in which solid oil lies very much of the virtues and qualities of the vegetable substance, which being separated from the salts of vegetables, they were found all alike; and that this only specificated them.

The Pulse.

The President speaking of the practice of the Chinese physicians observed, that they were extremely curious about feeling the pulse of the patient, examining the beating thereof, not only in the wrist, but in divers other parts of the body; by which they pretended to make great discoveries of the disease. And he was of opinion, that the ancients might know and make more use of the information of the pulse than our modern physicians of Europe; and that there might be more GALEN'S curiosity about pulses than was at present understood.

Mr. HOOKE was of opinion, that the pulse might discover somewhat of the state of the part, where it was, by means of the stopping of the blood; whether it were in the vein, artery, or muscular flesh, the artery being thereby more strained and extended.

Instrument for Drawing Archimedean Spirals.

Mr. HOOKE produced an instrument, and showed a way of exactly describing the spiral of ARCHIMEDES by a new property thereof, and that with as much ease and exactness, as a circle could be described; whereby not only a given arch might be divided into any number of equal parts, but a straight line given equal to the circumference of the circle.

Parabola.

Jan. 25. Mr. HOOKE showed a new method of describing a parabola, which on a plane is exactly as a circle by the help of compasses; which method he demonstrated to be geometrically as well as mechanically true: by which means he designed

to make a true gauge for the forming the shape of the specular concave.

Phosphorus.

Feb. 1. Dr. SLARE gave an account of some further experiments, which he had made with the new shining substance.[1]

Perpetual Noctiluca (Phosphorus).

Mr. HOOKE gave an account of the contents of a letter to Mr. HAAK from a gentleman of Berlin, mentioning, that Dr. ELSHOLTZ hoped, that he should soon have the perpetual noctiluca so as to enlighten the whole room, he being already able to read a large print by it.

Freezing.

Mr. HOOKE produced several eggs and apples, which had been covered with snow and salt all the day, in order to try the experiments, which had been formerly proposed by Mr. HENSHAW, viz. whether two of such frozen eggs or apples being at the same time put, the one into a glass of cold water, the other into a glass of warm, that, which was put into the warm, would not sooner freeze, and form a crust of ice about itself, than that, which was put into the cold. But two of them being so ordered and suffered to lie in the waters about half an hour, neither of them had contracted a crust of ice; which was judged to be occasioned by the warmth of the meeting-room, in which the experiment was tried.

Phosphorus.

Feb. 8. Upon discoursing further concerning phosphorus, there were several disputes, whether there were any such thing as *flammula vitae*: and it was conceived by some, that the experiments of phosphorous plainly proved such a *flammula*, as being extracted either immediately out of the blood, or mediately out of the urine. Mr. HOOKE was of opinion, that there could be nothing proved of that nature by it; for though by a certain preparation, a shining burning substance was extracted from it, yet it was possible, that as a shining and burning substance might be extracted out of other substances not animal; as was particularly proved by the shining of other substances, and particularly lime, formerly mentioned by the President, which was made of that quality by the violence of the fire: and for the quality of burning, he urged, that oil of vitriol and oil of

[1] An account concerning a further prosecution of experiments with the phosphorus by Dr. SLARE is printed in Mr. HOOKE's *Philosophical Collections*, No. 5, p. 84.

turpentine would produce very strange effects by mixture. He mentioned also an experiment with oil of turpentine and rectified spirit of urine, to show the strange motion and expansive power of liquors. It was thereupon desired, that he would show these experiments at the next meeting.

Hereupon was also occasioned a further discourse concerning the nature of fire, and how consonant experiments were to the theory of the dissolution of bodies by the air as a menstruum.

Ray and Willughby.

Feb. 15. Mr. Hooke was desired to inquire, whether Mr. Ray was in town; and, if he were, to inquire of him, whether he had any of the philosophical manuscripts of Mr. Willughby in his hands.

Turpentine and Alcohol and Sulphuric Acid.

Mr. Hooke showed the two experiments, which had been ordered at the last meeting: 1. The moving of oil of turpentine upon spirit of wine. In the first trial in highly rectified spirit of wine, the turpentine sank; but in brandy it succeeded well, and exhibited a great variety of motions. 2. The mixture of oil of vitriol with oil of turpentine, which grew very hot, and swelled much, as was expected, but not so much as had happened in several other trials.

Parabola.

The President being now present, and Mr. Flamstead having cavilled against the method shown by Mr. Hooke of describing a parabola, and affirming it to be false, the society desired Mr. Hooke to show again the way, which he had demonstrated at the last meeting: and which he now repeated, and demonstrated the ground thereof. Upon which the President declared to the society, that it was true and certain, and the best way yet known of describing that curve, and never published before.

Feb. 18, 1681/2.

Dr. Hook's Letter to Dr. Trapham, of enquiries for Jamaica.
[Derham, p. 64.]

Sir,

It will be a great obligation to the *Royal Society*, if Dr. *Trapham*, or any other ingenious and knowing person in *Jamaica*, will please to communicate any curious observations they shall make, concerning any part of nature; as concerning the temperature and qualities of the air, the seasons, winds, storms, hurricanes, rains, hails, dews, mists, fogs, *&c.* the heats, colds, &c. of the seasons;

the qualities of springs, rivers, lakes, *&c.* the description of any of the animals, birds, beasts, fishes, serpents, insects, or of any of their qualities or uses, for food, physick, pleasure, *&c.* The description of their vegetables; as of their herbs and shrubs, whether of the land or sea; of the trees; their use in food, physick, building, dying, perfuming, firing, joinery, turning, bows, *&c.* The description of any of their peculiar stones, minerals, ores, metals, clays, earths, sands, *&c.* of what nature, what use made of them, *&c.* Also to inform them concerning any accurate observations, that have been made of any eclipses of the moon, and particularly that of the 11*th* of this instant *February*; of the variation of the magnetick needle, from the meridian, or north point; of the times of the tides, both spring and neap, and of the height it rises; of the currents, what, when, which way; of the depths and soundings of the seas thereabouts, and whatever of this kind shall be communicated; or if any curious jewels, shells, seeds, *&c.* shall be sent, the society will not only pay the charge of freight, but any other way gratefully acknowledge the favour that the communicator shall desire, either by recording it in their registers, or publishing it in their histories. [R. H.]

Visit to Sir W. Jones about Arrears.

Feb. 22. Mr. HOOKE, in prosecution of the desire of the council, having attended Sir WILLIAM JONES with the state of the case concerning the arrears due to the society from several members, produced Sir WILLIAM's opinion thereupon.

Eymart's Sextant.

Mr. HOOKE showed a draft of a sextant made by Mr. EYMART for making observations of the late comet.

Leewenhoeck's Letter on Muscle.

Mar. 1. Mr. HOOKE produced a long letter from Mr. LEEWENHOECK, containing an account of several curious observations and discoveries made with a microscope by himself. The letter being in Low Dutch was not read; but Mr. HOOKE having translated half of it read it to the society, wherein was an account of several curious discoveries relating to the fibrils, hair or small claws of muscles. He promised to translate the remaining part of this letter against the next meeting.

Ellipsograph.

Mr. HOOKE showed a way of describing all varieties of ellipses

by a new sort of compasses invented by himself, in which he made use of the same instrument, with which he described the parabola and spirals.

Leewenhoeck's Letter on Muscle.

Mar. 8. Mr. HOOKE brought in the translation of Mr. LEEWENHOECK'S letter, which he read and explained some parts of it, and gave an account of what observations he had himself formerly made about the fibrils of muscles, their smallness, and form much like a chain of beads or a necklace of pearl; and he remarked, that he had several times written to Mr. LEEWENHOECK to desire him to inquire further concerning the nature of muscles.

Mr. HOOKE was desired to answer this letter of Mr. LEEWENHOECK, and to send him the *Philosophical Collections*, that had been printed, and to publish this letter in the next *Collection.*[1]

The extremely small stringy parts of the muscles of a lobster were shown by Mr. HOOKE in a microscope; whereby it appeared, that these filaments of muscles were not more than the tenth part of the diameter of the hair of one's head.

W. Briggs on Vision.

Mar. 15. Mr. ASTON read a paper of WILLIAM BRIGGS, M.D. concerning a new theory of vision.[2]

Radiant Heat.

Mr. HOOKE exhibited an experiment to show, that the heat of the fire was not propagated in the same manner as the heat of the sun; for that a plain looking-glass plate being put between the concave metal and the fire, though it seemed very little to hinder the propagation of light, yet it took off almost all the heat in the focus of the concave; as was experimented by several there present, and particularly by a nobleman of Savoy introduced by Sir THEODORE DE VAUX.

River Mussel and Worm Tubes.

Mar. 22. Mr. HOOKE brought in Mr. LISTER'S present for the repository of the shells of a very large thick river mussel, and a stone all over encrustated, wherein were some kinds of worms.

Notes in HOOKE'S *Diary*.

Mar. '82. NB. Double object glasse microscope, plano-convex.

[1] A copy of HOOKE'S acknowledgement of this letter on the muscular tissue of crabs and lobsters is in MS. *Sloane* 1039, f. 134.

[2] It is printed in Mr. HOOKE'S *Philosophical Collections*, No. vi, p. 107, for March 1681/2.

NB. ∴ Linseed and turpentine on water curiously coloured.

NB. Various sorts of microscopes by drops. A microscope by Drop exceeding small globular microscope by glasse hair.—burning glasse in lamp-frame.

1682

Tasman's Voyage. *Other* Voyages *to be Purchased.*

Mar. 29. There was another paper, which Mr. HOOKE would have read, viz. his translation of an account of the discovery to the southward of Nova Hollandia in the East Indies in the year 1643, extracted out of the Journal of Captain ABEL JANSEN TASMAN, and published in Low Dutch by DIRK REMBRANTSE: but this account[1] being somewhat long, it was deferred. However, Mr. HOOKE moved, that the society would take care to collect all such voyages, as had been already published, or that could by any other ways be procured. Whereupon it was desired, that Mr. HILL and Mr. HOOKE would endeavour to procure and buy as many of that kind, as they should meet with, for the use of the society, and bring in an account of the expense to the council.

LETTER FROM HOOKE TO MARTIN LISTER.

These for my honed freind Dr. Martin Lister at York.

March 30, 1682.

Sr. Your Presents of the Black muscle and the Pholados stone were very kindly received by the Society to whom I delivered them on Wensday last. But the curious observation and most ingenious reflections conteined in your letter being read yesterday did much surprise them with wonder and pleasure. Thereupon insued much discourse and debate concerning the hypothesis you have so plausibly solved it by. It does indeed in my opinion soe perfectly agree with the trouble and usual method of Nature in such kind of extraordinary or monstrous productions that I could not choose but with some pertination[2] defend the doctrine againe some very material objections alledged against it. I urged in favour of it that it seemed to me that all seed what ever received a qualification or modification from the womb in which it was fostered from the egg or seed till it become a creature sufficient to subsist of itself. Instances of this I urged in the

[1] It is printed in Mr. HOOKE's *Philosophical Collections*, No. 6, p. 179.
[2] Not in *N.E.D.*

production of vegetables, birds, beasts, insects etc & more particularly in the monsters produced by the copulations of creatures of differing species. But Sr., I fear I trouble you with these repetitions who doubtless can say very much more in favour of this doctrine than was urged upon this unpremeditated defense. Upon the whole the Society were exceedingly pleased, & desired me to returne you their hearty thanks both for your Discourse & your present, and twas inquired with some earnestnesse whether it would be printed in the Collection. My answer was yt I would consult your pleasure concerning it, which if you will therefore please to signify in your next, you will obleige Sr.

Yor very humble servt.

R. HOOKE.

Soe soon as I know by wt hands, I shall from time to time send you these collections so soon as published.

Muscle Fibre.

Apr. 5. Mr. HOOKE read a letter,[1] which he had sent to Mr. LEEWENHOECK concerning the discoveries, which he had made and shown the society four or five years before of the figure of the fibrils of the muscles of crabs, lobsters and shrimps, &c. viz. that they in appearance through a microscope resembled a necklace of seed pearl; and that every one of those fibrils, which was not much above a hundredth part of the bigness of an hair, seemed to be distinct strings of pearls or bullets; and that the whole bulk of the fleshy part of those muscles was made up of an infinite number of those fibrils lying parallel by one another; but that he had not hitherto been able to see that figure in the fibrils of the muscles of the flesh.

He also read a letter from Mr. LEEWENHOECK in answer to the same, dated at Delft, April 4, 1682, N. S., giving an account, that he had at the desire of Mr. HOOKE viewed the muscles of crabs and shrimps and had found the same appearance; but that he had discovered them to be of the same nature with the rimples in the fibrils of flesh muscles; and to be composed of other less fibrils, as are those of flesh.

The shape and structure of the fibrils of the muscles of a lobster were shown in a microscope by Mr. HOOKE; and plainly seen by several of the members present, who had had some doubts of the truth of the appearance, as having not been able at other times to discern them.

[1] See Mr. HOOKE's *Philosophical Collections*, No. 7, p. 189.

Tasman's Voyage.

Apr. 12. Mr. HOOKE gave an account of a voyage made by the Dutch in the year 1643 to the south of Nova Hollandia, showing the way, which they had taken upon the globe, together with an account of the most considerable remarks, which they had made on the variation of the needle, the hollowness of the sea, and the nature of the countries.

Italian Sand.

He also showed in a microscope a very curious sort of sand, brought out of Italy, and different in shape from all the other sand ever seen by him, being very fine and white, and yet all the grains thereof were of some round or oval figure, and none angular, as all the sand in England is.

Muscle.

He showed likewise part of a flesh muscle in a microscope; but the rimples mentioned by Mr. LEEWENHOECK could not be discovered, though examined by a very good microscope.

Whiteness of Hair.

Apr. 19. Mr. HOOKE mentioned the way of making a white star in the forehead of a horse by scalding out the old hair, or some other way making alteration there: That it is generally affirmed, that the plucking out hairs or feathers by the roots will make the hairs or feathers, that grow anew, to become white: That from the texture, which he had observed in a porcupine's quill, he conceived, that the texture of some sorts of hairs might be * *.

Catalogue of Benefactors.

Apr. 27. It was desired, that Mr. ASTON and Mr. HOOKE should meet on the Monday following to draw up a catalogue of benefactors.

Arrears.

Mr. HOOKE was desired, when he should meet Mr. COLGRAVE, to inquire of him how the arrears of the late Marquis of Dorchester might be procured from the Lady GRACE PIERREPOINT.

Apr. 28. *Letter from* TH. CROWLEY *to* HOOKE.

[MS. Sloane 1039, f. 124.]

Propagation of Light.

May 3. Mr. HOOKE read a discourse about the manner and reason of the propagation of light, whereby he explained the difficulties of DESCARTES' propension to motion, and Mr. HOBBES's

conatus to motion, by showing how they might both be understood to be actual local motion: which was done by showing what was to be understood by a human moment and a sensible space, and how much shorter moments, how much smaller bodies, how much shorter spaces, how much quicker motions might suffice to perform the several propagations of the local motions of light through a sensible particle of body thousands of various ways successively, without interfering with one another.[1]

This occasioned much discourse, and some difficulties supposed therein were removed by some further discourses thereupon.

Colours Produced by Chemical Action.

Mr. HOOKE showed some experiments of colours, as particularly the changes made into green by spirit of urine, and into purple by aqua-fortis put upon a blue syrup of violets.

He showed also the experiment with the tincture of *lignum nephriticum*, which by a solution of salt of tartar after some time standing grew thick and muddy, but by pouring in a little spirit of nitre, began to grow clear up from the bottom, and so made a representation of the changes caused in the air by the various steams arising out of the earth.

Barometer.

Hereupon Sir JOSEPH WILLIAMSON inquired, whether the barometer had been any further improved; and whether yet any certain theory had been founded.

Mr. HOOKE answered, that the barometer, that stood in the meeting-room, showed the alterations very sensibly; so that the alterations, which in the common barometer were but two inches, were by that made four feet; and consequently the thousandth part of the whole difference was made very sensible: that there had not been any more rules made different from those, which he had taken notice of twenty years before, viz. that in windy and rainy weather the quicksilver was lowest; and that in calm and dry weather it was highest: But the cause was not yet determined: that he had further observed the smooth under-surface of the clouds to be high in dry weather, and low in wet: that he conceived, that the lower region of the air might be increased or diminished by the steams issuing out of the earth at one time, and being soaked up or imbibed at another: that in thundery weather he supposed great quantities of hot sulphureous streams to issue out of the earth, which caused the sultriness that preceded: that he had often seen the lightning kindle at the surface of the earth, and dart up into the clouds; and had often seen the thunder-cloud increased by continued steams of vapours,

[1] See Mr. HOOKE's *Posthumous Works*, p. 129, et seqq.

like smoke rising under the cloud, and ascending into it: and that the clouds seemed to swim like froth upon the surface of several regions of air.

Tables for Arithmetical Calculation.

A gentleman recommended by Mr. BOYLE showed the society some tables of multiplication, by which he affirmed, that multiplication, division, and the extraction of the root might be much facilitated.

Mr. HOOKE, Mr. COLLINS, and Mr. CLUVERUS were desired to meet this gentleman at Dr. PELL's lodgings on the Friday following at four in the afternoon, and to draw up a report to the society concerning these tables.

Barometer.

May 10. The last minutes occasioned some further discourse concerning the barometer; and Mr. HENSHAW was of opinion, that the gravitation of the air was caused by new * * * from other parts to the upper regions of the air. Mr. HOOKE thought, that most of these changes might be caused, partly by the temperature of the seasons, as to heat and cold, partly by particular exhalations and breathings out of the earth itself, and the particular qualifications of the earth at that time; for that it was observable, that the earth at some times was more spongy and sucking than at others; partly also by the very nature of the air itself caused by such exhalations, being sometimes more apt to dissolve, at other times more apt to precipitate moisture.

Discourse on Local Motions.

May 17. Mr. HOOKE read a discourse of his own about local motions, sensible and insensible times, and celerity, being a further continuation of the discourse read by him to the society in the meeting of the 3rd of May, which was well approved.

[This was his last Lecture on Light.[1]]

Metallic Glazes, Opal Glass, and Plated Wire.

May 24. Mr. HOOKE showed an earthen vessel, which seemed as if it had been covered with burnished copper: but he conceived, that there was nothing of metal in it, but that it was a peculiar quality of the glazing, which made it lose of that colour: that he knew the way, how to make * * to look like silver or white metal: that there were several ways with little or no addition to change the colour of glasses very considerably; and thereupon he showed a piece of glass, which gave a purple, blue, green, yellow, red, according as it was held various ways to the light.

[1] *Posthumous Works*, pp. 138–47.

He also showed a piece of glass, which he had newly changed into an opal in a very little time.

Hereupon the way of making chalcedonies and opals and * * *, was discoursed; as also concerning the Indian gold thread, the covering of which is of plated wire, but drawn finer than the hair of a man's head, and flatted and twisted about silk, as over gold twist; so that some were of opinion, that it was only a varnish: but Mr. HOOKE affirmed, that he had found it to be covered with a flatted wire, which he conceived to be gold. Sir JOHN HOSKYNS supposed, that it might be some other factitious metal, and that because he had been informed they could do such things, and had likewise known a gold-beater in Green Arbour Court in the Old Bailey, who every day made a quantity of a certain metal for his trade, by which he could get ten pounds in a morning.

Pendulum Clocks.

June 7. Hereupon was occasioned a discourse about the motions of pendulum clocks, concerning which Mr. HOOKE affirmed, that he had in the year 1664 read some discourses before the society, that the length of pendulum clocks in several countries ought to be different, and therefore that the pendulum clocks could not be fit instruments for keeping time at sea sufficient to discover the longitude, though they could be carried never so steadily; and that he had since been assured by several persons from several places, that there was such a different motion of them, that in places more towards the line they were to be made shorter to keep the same time: and that Captain SHEERES had newly confirmed that observation, by relating his own experience at Tangier with a pendulum clock adjusted in England, before he went; which he had found to move there too fast; and therefore was obliged to make his pendulum longer considerably.

Comparison of Standards of Length.

Mr. HOOKE mentioned Monsieur PICART as having taken much pains in comparing the measures of the length of other countries with the Paris foot in his book concerning the measure of a degree upon the earth.

Measurement of a Degree of the Earth.

Hereupon the President inquired of Mr. HOOKE the reason, why the measure of a degree upon the earth was not taken here in England, as had been formerly desired. To which he answered, that if the society would defray the expense thereof, he was willing to take care of it, and to see, that it should be accurately

done: that the French in their experiments had made use of some of those means, which long before they had undertaken it, himself had propounded and discoursed of to the society; and that in the use thereof they had doubtless been very accurate, as appeared from the account, which they had given thereof in Monsieur PICART's book; by which notwithstanding it was evident, that they differed very little from the length of such a degree long before examined in England by Mr. NORWOOD between London and York: that therefore, if the thing were to be done now again here, it would be necessary, that yet more accurate ways should be made use for performing it; or else it would be but *actio acti*: that it would be necessary both for this and any other accurate trials, that the society should have in their custody an accurate measure of the standard foot of London: and that for other experiments also they have the true weights and measures of England, with which to make the comparisons of other weights and measures. Whereupon it was desired, that Mr. HOOKE would take care to procure those for the society, that they might be always ready to have recourse to, when there should be occasion.

Discourse on Memory.

June 21. Mr. HOOKE read a long discourse, being the substance of three lectures, which he had missed the reading of at two last meetings, concerning the means, how the soul becomes sensible of time, explaining the organ of memory, and its use for retaining and producing ideas therein stored up.[1]

June 28. The minutes of the preceding meeting being read and discoursed of, there being several persons present, as Sir JOHN HOSKYNS, Sir WILLIAM PETTY, Sir ROBERT SOUTHWELL, Mr. HENSHAW, Monsieur JUSTEL, Monsieur AUZOUT, and others, who were not at the last meeting when Mr. HOOKE's discourse was read, it was desired by them, that Mr. HOOKE should read the same again, which he accordingly did.

After which some objecting, that this discourse seemed to tend to prove the soul mechanical, Mr. HOOKE answered, that no such thing was hinted, or in the least intended in it; it being only designed to show, that the soul forms for its own use certain corporeal ideas, which it stored up in the repository or organ of memory, and that by its power of being immediately sensible of those ideas, whenever it exerts its power for that end, it thereby becomes sensible of those ideas formerly made, as if they were made at that instant, but with this difference, that the farther they were removed from the centre or seat of its more immediate momentary residence, the more faint are the

[1] See his *Posthumous Works*, p. 140, et seqq.

reflections or reactions from them; and that this occasions the notion of the distance of time.

Demonstration of Archimedes Problem.

Mr. HOOKE showed a very easy way of plainly demonstrating the problem of ARCHIMEDES, whereby he proved, that the tangent of his spiral at the point of one revolution from the centre will cut a ray produced from the centre at right angles to the ray from the touched point; so that the part between the point of intersection and the centre shall be equal to the circumference of the circle, that passes through the point of the spiral, where the said tangent touches it; which was performed with any supposed *praecognita* in geometry known.

Hooke Desired to Print his Works.

July 5. The minutes of the preceding meeting were read and discoursed of, and the society desired, that Mr. HOOKE would, with what convenient speed he could, print his discourses and lectures read before the society; as also a more full description of all those several instruments, which he had shown that year to the society, together with the demonstrations of the grounds and reasons and use of them; which, he said, he was willing to do as soon as he could find leisure to fit them for the press.

Archimedean Spiral.

He also more particularly explained that problem, which he had shown at the last meeting concerning the nature of the spiral of ARCHIMEDES, and the tangent of any part thereof.

Hooke's Arrears.

July 12. It was ordered, that Sir WILLIAM PETTY be desired to accompany those members of the council, who by a former order were desired to speak to Sir JOHN CUTLER for obtaining Mr. HOOKE's arrears.

On Matter.

Mr. HOOKE read a discourse concerning the unlimited extent and divisibility of matter, showing the bulk of a man to be in a medium between them; and how far endeavours hitherto used had informed us concerning the media between them; and how much further mechanical contrivances could yet advance us beyond the imaginations of most men hitherto known.

Digges and Bacon. Invention of Telescope.

July 26. Mr. HOOKE read a passage, that he had met with in DIGGES's *Stratioticos*, wherein he affirms, that his father had a method of discovering all objects pretty far distant, which lay

round about in the country; and that this was by the help of a book or manuscript of ROGER BACON of Oxford, who, he conceived, was the only man besides his father, who knew it. This was the more remarkable; for that this *Stratioticos* was printed in 1579; which was more than thirty years before METIUS or GALILEO made their discovery of those glasses: and therefore it seemed evident, that ROGER BACON was the first inventor of telescopes, and LEONARD DIGGES the next reviver of them, both Englishmen.

Dr. PLOT being present said, that he conceived, that this book of ROGER BACON, which accidentally fell into the hands of LEONARD DIGGES, might have been in the custody of Mr. THOMAS ALLEN of Gloucester Hall in Oxford.

Mr. AUBREY remarked likewise, that there was a passage in the preface of LEONARD DIGGES'S *Pantometria*, which mentions the same thing to be known by him; and that he had shown it to divers; which book was printed some time before the *Stratioticos*.[1]

Chinese Distorting Mirror.

Mr. HOOKE showed a Chinese perspective box, in which by help of reflection the room and figures were considerably lengthened.

Borelli's Theories.

He also gave an account of the theories contained in the second volume of BORELLI'S book *de Motu Animalium*.

Experiments on Colour. Projection of Images on Paper.

He showed two optical experiments in a darkened room, the one of the succeeding of colours, viz. yellow, green, and blue; and the other of making the representation of a man's face and body, or any such object, of any convenient bigness, so as to be able to delineate the same exactly upon paper, cloth, or the like.

Aug. 16. HOOKE *Observed the Comet.* [*Post. Works*, p. 159.]

HOOKE'S *Diary*. [MS. Sloane 1039.]

Teand Sug. corroding. N.B. Melancoly M. Qu. idem de O. & S.

July 18. Teles[cope] by press of water.

July 29. Idem ground when exhausted.

Aug. '82. Printing with plaister and cement found. Sir Chr. Wr[en] time problem found.

Sept. 9 found ∴ □ to cleanse speculum try'd the speculum for gtant with good success. Rd. Sturring's paper.

[1] It was not printed till twelve years after.

NB. To get a parapet wall between the stables and the gallery against pier. ∴ ± takes off tarnish of Tin and gives true colour.

Michaelmas Day. Demonstrated Rhum line on a cylinder projection. The pole the first parallel, meridians and parallels being straight lines, the curve is Descartes Line and is easily described on a plane: it always cuts a parabola at right angles. Measuring distance difficult but not impossible the parallels differ half tangents from the pole. About a fortnight since I perfected Sea Waywiser and fitted it for wind, water and magnet.

Oct. 1. Borrowed of Mr. Pos. Barron de mirabilibus Artis 4to. 112. Pomp Gauricus et 4to. Returned ye chinese book and Mr. Witton's book. Returned them.

Oct. 2, '82. Measure ⊖. Quicksilver will 100 lb level telescope. Distance polar point.

Polar glasse·36 foot, throug floor fix iron etc.

Oct. 3. Suspend a long wire: observe its shrinking and stretching by index and weight.

Oct. 6. Demonstrated geometrically the true straight Rhum line on a cylinder from the inclination of the planes: the eye always supposed to move.

Oct. 14. Instrum[ent] for degree by reflex ☿ at well or colum bottom viewed from top by short telescope. Star covered by hair near eyeglasse. Or, by 400 ft. object glasse in the same circle with eye glasse: the former possibly best and easiest.

The same done by Plano-reflex fixed on stone, telescope horizontal of 1, 2, 3 or 400 foot length.

Alteration of Pole point *eodem modo*.

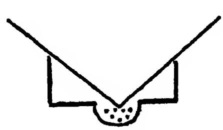
Oct. 17. *Cito mane.*

1. Way to leap by spr[ing] shoes 12 foot perpendicular, and 20 ft. parabolical by Repetition rising bell-like to a great height and as Raised (?) intended by being screws.

2. By a telescope of 1000 ft. fixed horizontally and directed to zenith Pole etc. by Reflex plano metal.

3. Lens for Object gl[ass] or Eye gl[ass] of Oyld Drumhead blown up with Oyle: quere, horn Muscovy glasse, glew etc.

4. For perpendicular at sea ☿ in the box, and seen by the two perpendicular rules, and *β*. *Z* .



5. Perpendicular telescope by the pressure of water on a plano levell glasse.

Arrears.

Oct. 25. Mr. HOOKE to find bonds of Fellows whose dues were in arrear.

Discourse on Comets.

Mr. HOOKE read a discourse concerning comets,[1] and in this first part of it gave an account of several of his own observations concerning the appearances of the comets in 1680 and 1681; in which he mentioned several new and wonderful appearances of them, taking notice of the other remarks concerning them, as of their place, position, magnitude, motion, way or course, only in short, and by the by, referring his observations in those particulars to the other parts of the discourse.

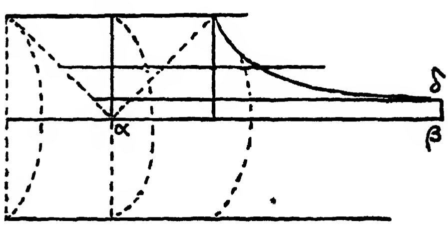
Cylindrical Projection of Sphere.

Mr. HOOKE showed a new projection of the sphere into a concave cylinder, which could be opened into a plane, in which he described the rhomb lines geometrically and mechanically by a very easy instrument, and very fit for the use of navigators.

HOOKE'S *Diary*. [MS. Sloane 1039.]

Oct. 27. Q. What alteration to an animal in rarified and condensed air suddainly and often changed by air pump.

Oct. 27, '82. The areas of the Rhoms are the hyperbolick areas on the superficies of a cylinder. G. St. Vincent divides areas of the hyperbola q these areas



α β axis cylindri *Z* apex hyperbole.
z δ hyperbole dimidium cuj *α δ* asympto

Oct. 30, '82. The steersman of a ship to sit above and by an engine manage the rudder and care the ship.

Item by levell (?) fins at the keel to keep a ship from dipping into the water before, and also by sayles.

Nov. 1, '82. Qu. Rhumb lines on plain chart, hyperbolas.

[1] See his *Posthumous Works*, p. 194, et seqq.

Engravings of Birds.

Nov. 1. Mr. HOOKE delivered in the pictures of five strange birds, which had been taken at Nuremberg in Germany, and there delineated and engraven in copper. They were presented by Mr. ARNOLDUS, who had lately received them from Nuremberg. 1. An Onocratulus. 2. Scherben or seerab, as it is called in High Dutch, a kind of Soland goose. 3. A strange kind of breasted duck. 4. A kind of bittern. 5. A sort of bustard. They were ordered to be kept in the library with Mr. WILLUGHBY'S *Ornithologia*.

Variation.

Nov. 8. Mr. HOOKE related that he had lately got an observation of the magnetic variation to be made at Lisbon, and expected to receive an account for Ireland.

Millepedes. Longevity.

He also read letters from Dr. CARTE of Manchester on live Millepedes vomited by a patient;[1] from a friend on the anatomy of a bat, and of a lamb that had been taken out of a ewe, which had been generated *extra uterum*, and of a Cornish woman lately deceased of the age of 154 years.

Comets.

Mr. HOOKE read part of a further discourse concerning his observations and theory of comets; and the remainder of it was referred to another meeting.

Longevity at Hudson Bay.

Nov. 15. Mr. HOOKE took notice of what Sir CHRISTOPHER WREN had formerly acquainted the society, that the people at Hudson Bay commonly live to 120 or 130 years of age; and till that age are very lusty, and commonly go to hunting, which when they are no longer able to do, they usually invite all their kindred, and lie down and resign themselves to be strangled by the eldest of those, who survive, and who takes the care of government in his father's stead.

Comets.

Mr. HOOKE read a further discourse concerning comets, therein explaining how it may be supposed, that comets are burning bodies, and that the blaze of them is made partly by steams into flame: that all these steams do proceed from a small solid body or star actually on fire, which is enveloped with a white cloudy body of steams, or smoke, which make up the

[1] *Phil. Experiments and Observations*, p. 75.

appearance of the nucleus in the middle; from which doctrine he deduced several conclusions, which he undertook to explain more fully hereafter.

The Late Comet.

Nov. 22. Mr. HOOKE fetched his observations on the late comet and showed the figure of it taken by himself on the — of August, having carefully observed it with a 14-foot telescope, and delineated it with his own hand.

Election of Officers.

Nov. 30. The society met upon summons, as usual, for electing the council and officers for the year ensuing.

Dec. 6. Mr. HOOKE delivered up the bonds to Mr. HILL, the key of the chest and the council books to Mr. ASTON; and the key of the press to Dr. PLOT.

'Guinea and Feather' Experiment to Illustrate Possible Resistance to Comet.

Dec. 20. Mr. HOOKE brought in a tall exhausted glass with a feather enclosed, which appeared to fall from the top of a glass with a velocity equal to that of a more solid body in the air; but the same feather upon readmission of the air fell wavering and slower by many degrees. The intention of this experiment was to show the easy divisibility of the body of the ether; that though the motion of a comet be swift, yet the resistance made to its motion is but small: and that though the flame of a comet gives a little way to the ether, yet it does not follow it, like that of a candle moved in the air, being a grosser body.

Nutrition of Embryo.

A letter from Dr. TYSON to Dr. PIT was read, containing an account of the dissection of a monstrous lamb large and well grown; but when it was yeaned, not having a mouth or other passage to the oesophagus.

From hence it was questioned how this lamb was nourished in the uterus, it being the received opinion, that the foetus takes in its nourishment by the mouth, which Dr. TYSON confirmed by several arguments.

Mr. HOOKE was of opinion, that the blood contributes much to the nutrition of an embryo; and in this case of the lamb there was no doubt made by any one.

Respiration.

Several things were mentioned of Mr. HOOKE's experiment of blowing into the lungs of a dog, which keeps the animal alive, not by raising the lungs to make the blood circulate, but perfecting the blood by the nitrous particles of air, which have

that efficacy, as to make the blood come out of the lungs an arterial blood, which entered into them a venal.

Dec. 27. HOOKE prepared an abstract of L. F. MARSILI'S *Osservazioni interno al Bosporo Tracio*, from a copy borrowed of Dr. CROONE and returned this day. [MS. Sloane 1039, f. 135.]

1682/3

The Secretary's Papers.

Jan. 3. The Secretaries having drawn up a state of the papers in their office, and it appearing, that several things of the society's had not been put into their hands, as ought to have been; it was ordered,

That Mr. HOOKE deliver up into the hands of either of the Secretaries all such books and papers, as any way belong to the society, or came to his hands upon the account of his having been Secretary.

Cause of Whiteness.

But the considerations of Mr. LISTER and Dr. GREW having been upon the particular production of whiteness in bodies by oils or salts, Mr. HOOKE observed in general, that whiteness proceeded from the reflection of multiplicity of rays: that therefore glass, though of itself transparent, yet when beaten to a small powder, is white; that blue glass is the same, and that ultramarine, the strongest blue, when very finely ground, makes a whitish powder: that in liquors two heterogeneous bodies (not mixing well together) one of which is transparent, the other beat into small balls, make a white by the reflection and refraction of more rays from the little opaque bodies.

Radiant Heat.

Mr. HOOKE remarked, that black and white marble being exposed equally to the fire, the black is much hotter than the white; because this reflected back the rays, which the other did not: and that a white marble or stone, if one half of it were coloured black, would, when exposed to the fire, be much hotter in the black part, than in the white.

Colour of Sponge.

Mr. FLAMSTEAD queried, why a sponge is not blacker, since it is so full of holes, that absorb the light.

Mr. HOOKE answered, that though the hollownesses were many, yet there were also many reflecting and refracting parts in a sponge, which he had often observed to consist of many clear pipes a little inclining to yellow. Other reflecting parts in it were many, as he had often observed in its texture.

Gravitation in Aether.

Jan. 17. The first experiment being the proof of the gravitating of bodies in the aether, as much as the air was made; for which purpose Mr. Hooke brought in a long narrow glass well exhausted, having a long spiral wire fastened to the top, which bore an empty glass ball hanging at the end of it. The glass had a ring drawn round just at the fastening of the glass ball to the wire, that the true place and seat of the ball might be observed: which being well taken notice of, the air was then let into the receiver, and the situation of the ball observed to be still the same it was before.

Heat.

The second experiment was to show the different susceptibility of heat by a white and black body: for which purpose Mr. Hooke brought in a white marble stone, the one half of it being coloured with a black colour. When the stone had been a good while exposed equally to the fire, though the black part seemed to most present to be the hottest; yet it was agreed, that if the stone were very much heated through, the white would appear the hottest, as touching the hand with a more close and even surface.

Solubility of Sand.

Jan. 24. The dissolubility of sands in water from Staffordshire brine as attempted, it was conceived not practicable; and Mr. Hooke said, that nitre might be mixed with sand, and baked in an oven so as to lose its saltness, and become a perfect stone no ways dissolvable in water.

Brine Animalcules.

Upon mentioning small animalcules found in Staffordshire brine, and passing through the finest strainer, it was said, that there had been animalcules found in sea brines made by the sea at Lymington.

Aurelias.

From hence a discourse happening concerning animals in pepper-water, Mr. Hooke gave notice, that he had observed them turned into aurelias, which he would show the society at their next meeting.

Barometer.

Mr. Hooke showed an experiment of raising the spirit of wine much higher than in the ordinary barometer, made with quicksilver and spirit of wine.

Recording Barometer.

Feb. 7. Mr. HOOKE brought in a way of measuring the rise and fall of quicksilver in the barometer upon a spiral line, of which he was ordered to deliver an account in writing, that it might be considered and registered.

Cause of Gravity.

Feb. 14. Mr. HOOKE brought in a method of explaining the cause of gravity, an account of which he was to give in writing.

Black Lead Mine.

Feb. 21. Mr. HOOKE mentioned a mine of black lead in New England.

? *Cartesian Diver.*

The experiment was made of a body poised so, as just to be covered with water, which by a knock at the bottom of the vessel was made to sink more into the water: an account of which was expected from Mr. HOOKE.

Preservation of Mathematical Instruments.

Feb. 28. A proposal being made, that the mathematical instruments or engines, which had at any time been made by the society, or given to them, as also the apparatus made use of in the making experiments, might be taken an account of, brought together, and carefully preserved; it was ordered accordingly,

That Mr. HOOKE be spoken to, that he give the best account he can of them at the next meeting of the council, there being many of them said by the President to be laid together in an upper room.

Classification of Stones.

Mar. 14. Mr. HOOKE distinguished stones into those, that had a bituminous and sulphureous cement, and those, that had a saline:

That the bituminous burnt to lime:

But that the saline turned hard:

That marble was a bituminous, but Portland stone a saline:

That loam was a mixture of sand and clay:

That clay was fittest for making bricks; and so used by the Romans; but required a long time for the making them: that the clay must be laid open to the air and frost to have the body of it opened:

That stones, that are flaky, ought to be laid in the building as they lie in their bed; else they will be apt to moulder and decay:

That some stones, that cut firm, yet by lying in the air, dissolved, seeming to be made of a vitriolated salt.

Mr. HOOKE observed, that things, which had suffered the fire, were apt to attract.

Mr. HOOKE said, that he took volatile bodies to be such, as mixed easily with the air: that the more easily they mix, so much the more volatile they are. He named three degrees of them; some, that mix with cold, some with a tepid, and some with a very hot.

Fixed bodies he understood to be such, as will not mix with the air.

Heating of Salts.

It being queried at the last meeting, whether salts not having suffered the fire, would heat by being mixed;

Mr. HOOKE mentioned brass lumps found in coal-pits, being a kind of vitriolate salt, which have been said to take fire by rain and weather, burning the houses, where they have been laid.

He also named fermenting substances, such as hay.

Vibration Experiment.

Mr. HOOKE brought in an experiment for proving an attraction from the surface of a glass of water to the place struck with a fiddlestick on the side; an account of which was directed to be brought in.

Dr. HOOK *of Earths, Salts, &c. March* 14, 1682/3.

[Derham, p. 87.]

The nature of clays, stones, limes, *&c.* being discoursed, I mention'd the sorts of stone which were here call'd freestone, *viz.* such as could be saw'd with a tooth'd saw, such as *Cone, Rigate, Burford, Ketten, &c.* That stones were of two natures, one bituminous, or sulphureous, the other saline and watery; the sulphureous would calcine into lime, the saline make glass, vitrify or dissolve, and moulder with the rain, air, and frost. That both these sorts are often found in the same *Portland*-stone one part whereof will moulder, the other harden with the air. That loam is a mixture of various sorts of clays and sands, and may be separated by washing. That such a material is usually chosen for brick-earth, as being most easily softened and temper'd for moulding, and most easily and speedily dry'd for burning, and most easily burnt; to make it yet more easy for burning, 'tis usually dry, and exposed to the winter rains and frosts, for

mellowing against the spring. That the finest clay would make the best bricks, were it not for the more than ordinary labour and charge in washing, working, moulding, drying, baking, as is evident in pottery, and tiles, and especially in the *Roman* bricks, which are some of them of so fine an earth, so well moulded, and so thoroughly burnt, as to last even to this day, as intire and perfect as when first made, in all probability. That hungry clay was hardest and best to endure the fire without melting, but saline, and fine clays, were most apt to vitrify: and thence the throwing in of three or four shovels of salt into a pot furnace when hot, made all the pots in the furnace to be glaz'd. That *china* was such an earth, as was very difficult to be vitrify'd.

Concerning salts, and other volatile and fix'd bodies, I mention'd, that there were two sorts, one that was homogenous to the air, and would be dissolved into it. This was call'd volatile; the other heterogeneous, and would not at all be so dissolved and mixed with it; and these were call'd fixed. Of the volatile, there are various sorts, which will be dissolv'd into the air, by differing degrees of heat. Spirit of wine, or such other fermented spirits, camphire, the odorous gums of flowers, and herbs, will be dissolv'd into the air with a small degree of heat; other bodies more difficultly, and require a stronger and stronger heat, as they are more and more fixed; so some salts and gums, *&c.* will not rise at all: and these are call'd fixed bodies, or alcaly salts. Of these which are dissolv'd into the air, some are tasted as it were, by the nose, others not in the same manner as in tinctures made in waters; some, whereof the tongue does taste, others not.

Concerning the *Oxford* trial by blue starch, which they affirm'd would turn red, with acids, I said 'twas impossible, smalt being glass, but it must be *litmus*, or *indico:* but most likely *litmus*; being a clear, blue tincture; but *indico*, a thick precipitation.

The experiment was very considerable, though plain, giving a further explanation of gravity, by making a large glass vibrate, with a viol bow: by which vibration, a certain undulation is plainly seen to dart out from all such places where the glass vibrates. And it was very plainly visible, that the water, and bodies in it, did move towards every such vibrating part, and from every other part that was at rest.

1683

Navigation.

In 1683 HOOKE delivered a course of Lectures relating to the improvement of Navigation. They are printed among his *Posthumous Works*, pp. 451–572.

Journal Books.

Mar. 28. It was ordered, That any three or more of the council, then present, or of any other members of it for the present year, be a committee to meet at the repository, and inspect the Journal Books, to note any vacancies, omissions, or mistakes; or, Mr. HOOKE being present, to mend them, and supply them, with his consent or opinion, and on just occasions alter and strike out such places or passages, as he and they shall agree to; and in case of disagreement, make report to the council, that they may take further order therein: and if the omissions and vacancies be such, as could not be supplied, then to draw lines there in void spaces, that for the future there may be no new thing written therein.

Magnetical Experiment.

Mr. HOOKE was ready to bring in a magnetical experiment; but it was deferred till the next meeting.

Apr. 4. Mr. HOOKE exhibited an experiment tending to explain magnetism:

There were two flat pieces of wood like rulers, the one having a pin fastened in the middle; the other was in the middle suspended by that pin like a needle of the compass. By knocking gently on the end of the lower piece of wood, the upper piece moved towards the line of knocking, or not a quarter of the compass from it. The application of this experiment was not declared.

Squaring the Circle.

Apr. 11. Mr. HOOKE brought a draft in perspective of a square said to be equal to a circle.

Blue Pigment from Copper.

Apr. 18. But there being little answered about smalts, Mr. HOOKE said, that out of a copper mineral calcined and powdered, and mixed with sand, is made a bluish glass, which being quenched in water, cracks and crumbles into a fine blue powder.

Payment by Results.

June 6. It was resolved, that Mr. HOOKE shall receive every meeting-day order for the bringing in two experiments at the next meeting-day, together with a declaration by word of mouth of the purpose and design of the experiments, and an account in writing of the history thereof, and the purpose as aforesaid, such as may be fit to be entered in the Register: and that at the end of every quarter there shall be a meeting of the council, where his performances shall be considered, and a gratuity ordered him accordingly; and that from this time he have no other salary.

The Secretary was ordered to leave a copy of this order at Mr. HOOKE's lodgings.

Conditions of Office of Curator.

June 20. Mr. HOOKE being called in, and the order made concerning him on the 6th of June being read, he then declared his satisfaction therewith, and his resolution to proceed in his office of curator upon those conditions.

Access to Journal Books.

Leave was given to Mr. HOOKE to have access to the Journals or Council Books upon occasion of his business with Sir JOHN CUTLER.[1]

Lapis Lazuli and Gold.

June 27. In speaking of bodies, wherein gold is found, Mr. HOOKE said, that lapis lazuli was copper or the pyrites.

Mr. HOOKE said, that there had been some [gold] taken up in the west parts of England.

Petrified Mushrooms.

There being mentioned some Bohemian mushrooms containing gold, as was certified in a letter from Nuremberg, Mr. EVELYN said, that he had a very perfect mushroom petrified, which he took to be a field mushroom. Mr. HOOKE was of opinion, that the greatest part of the petrified mushrooms grew in the water.

Density of Melted Lead.

The first experiment brought in by Mr. HOOKE was the showing how a piece of unmelted lead would swim in melted lead.[2]

[1] Who had refused paying Mr. HOOKE his salary for reading the Cutlerian lectures.

[2] Register, vol. vi, p. 65. It is printed in his *Philosophical Experiments and Observations*, p. 89.

Upon this it was discoursed by some, that the air keeping to the unmelted lead hindered the melted lead from taking it in.

Surface Tension.

It was also said, that the surfaces of bodies often bear up a weight, which will sink, when it is emersed.

Dr. Hook's *experiments of the floating of lead, &c.* July 4, 1683. [Derham, p. 89.]

Wednesday, *June* 27, 1683, I shew'd two experiments to the Society, which succeeded; of which I gave an account, *Wednesday, July* 4, 1683, as follows.

Of the floating of unmelted metal, upon the same melted, with the cause.

I. There was melted, in a crucible, about a pound and half of sheet lead, and whilst it remain'd melted, several small pieces of the same lead were gently one by one, by the help of a *forceps*, laid upon the clear and bright surface thereof (the scum and litharge being first removed) and it was found that they all swam upon it, and did not sink to the bottom; but if they were all cover'd or plung'd under the surface, they would not rise again, but sink to the bottom, and soon be melted.

The occasion of the experiment, was a suggestion, that lead, when it concreted, did (as water when it congeals to ice) settle itself into a more rarify'd Texture, than when fluid; by which means, it became lighter than the melted lead, and so swam at the top of it. But though the effect were answerable to the assertion, yet the cause, assign'd, was false; for it was very evident, that the reason of its swimming, was much the same with that of the swimming of a needle, or of water-spiders, and many other insects upon the surface of the water; namely, a coherence of the air to the surface of the swimming body; which coherence of the air does depress and remove a greater part of the fluid, lead, or water, than the meer bulk of the body itself would do; which, in both these cases, is very evident; and was, in these trials, very remarkable; for the surface of the lead did plainly bend and sink below its level, with a roundness where the piece of lead lay; which bending of the surface, was made the greater by a thin plate or skin of litharge; which the air does presently make upon melted lead, so soon as ever a former is remov'd or scummed off.

Dross of Metals.

Upon discoursing of the dross of metals, Mr. HOOKE took notice, that iron upon heating turns it in a scale; and so as often as it is taken off; but that the same scales are easily reducible to iron.

Upon mentioning the rising of ice in rivers upon a thaw, it was conceived, that rivers do not freeze at the bottom; but if ice were accidentally carried down to the bottom of a river, the same in a thaw growing like a honeycomb would certainly rise up again.

Rising of Water.

The second experiment of Mr. HOOKE was to show how high water will rise in any place.[1]

Of the Condensation of Air by Water.

II. There was stuck into the side of a piece of wooden pipe, for conveying water, a small cylindrical pipe of glass, about a foot long, and somewhat better than half an inch in diameter; one end of which pipe was hermetically seal'd, but the other end was open, and communicated with the cavity of the wooden pipe, by means of a small hole bor'd in the side of that wooden pipe, into which the open end of the glass pipe was thrust hard, having a little linnen rag wrapped about it, as is usual for taps put into the end of a barrel, or other vessel. Then (there being about a foot of air left in the glass pipe) water was forc'd into the wooden pipe by a small force-pump; and it was plainly to be seen, that as the water was more and more strongly forc'd into the wooden pipe, the air left in the glass pipe, by the water that enter'd into it by the aforesaid hole, was condensed into a lesser and lesser room; so that hereby, the true degree of the pressure of the water could be easily found and measured; which was conceiv'd to be an experiment, or instrument of great use for water-works, because by means hereof, the force of water, in any pipe, might presently be known; namely, both from what height it descended, and to what height it would there again rise. The rule of doing which, was the next day, to be brought in.

Rising of Water and Specific Gravity of Molten Metals.

The experiments appointed for Mr. HOOKE at the next meeting:

1. A scale to measure the force of mounting waters.

[1] Register, vol. vi, p. 66. It is printed in his *Philosophical Experiments*, &c., p. 90.

2. An experiment to examine the specific gravity of melted metals.

Pressure of Water in Pipes.

July 4. Mr. HOOKE showed the rule for calculating the pressure of water in a pipe.[1]

Dr. HOOK's *two experiments, showing the pressure of water in pipes, and how to measure it. Also the expansion of melted metals, made before the Royal Society.* [Derham, p. 91.]

July the 4th, 1683. I read the accounts of the two experiments made *June 27*; and likewise further explain'd the uses of them, by discourses in other particulars, namely, that the second experiment was of great use for the trying the strength of pipes, for conveyance of water. By which means, I have examined several sorts of earthen and other pipes and cements, and have found that earthen pipes, made of a material only, as hard as house-tiles, would endure the pressure of 100 foot of water; that the use of the other experiment, was chiefly luciferous, namely, to shew the nature of fluids and congruity, of which I should shortly have occasion to discourse more at large.

Then I produced and read the rule, according to which the pressure of the water, in any pipe, might, by means of a trial with the former instrument, be calculated and reduced to certain measure in feet and inches. The means of performing, I shew'd, were principally two, first arithmetically, and secondly, mechanically.

The arithmetical rule was this; that the length of the cylinder of the air in the pipe, before it was press'd upon by the water in the pipe, should be compared to the length of the cylinder of the same air, when compress'd by the water of the pipe, and the difference noted; namely, the length of the cylinder of water thrust into the pipe, by the pressure. Then to resolve this proportion. As the length of the cylinder of water thus compress'd, is to the length of the cylinder of water so thrust in; so the height of the standard of water, at the time of trial, to the height of the cylinder of water pressing in the pipe, which is equal to the height

[1] Register, vol. vi, p. 68. It is printed in his *Philosophical Experiments*, p. 91.

to which the water of that pipe, so press'd, will ascend above the surface of the water in the small pipe.

The height of the standard of water, at the time of trial, is easily known by the height of the mercurial standard at that time; which, being now grown very common and useful, is almost every where to be met with, and many otherwise be easily supply'd; for as the weight of water, to the weight of quicksilver, so the mercurial standard, to the height of the water standard.

The weight of water, to that of *mercury*, is by many trials found to be near as 1,000 to 13,593, or as 1 to 15, according to his account following numb. - - - -

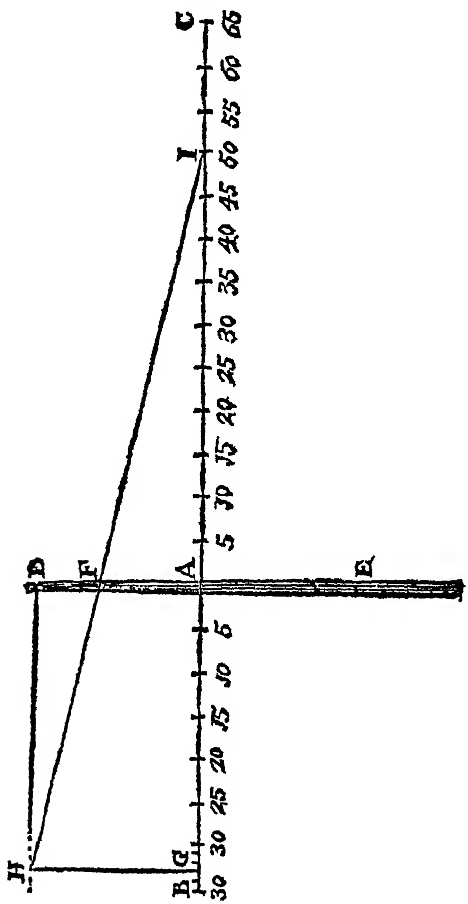
The geometrical, or mechanical way, was this. Upon a table, or plane draw a line, as B A C; then cross it at right angles, with another right line, as D A E, then divide A B, into thirty-six parts, and continue the same division, from A towards C, so far as you have occasion of foot heights of pressure; as suppose to 100; then subdivide one of these parts, lying next to A into twelve equal parts. Then knowing the present water standard, count, from A towards B, so many parts and duodecimals, as it is then feet and inches: cross the line, at that point at right angles, with another line, as G H, and from G, set off the length of the cylinder of air in your glass, before compression; then set off the length of the additional cylinder of water, from A towards D, as suppose to E, and laying a rule over the points H and F, see where it crosseth the line A C, as at I, then count the parts and duodecimals from A, and that shall give the pressure or additional height of the water, above the level of the water in your water poiser in feet and inches: the reason of all which depends upon the reciprocal proportion of the strengths of air to the extensions thereof.

He showed likewise a way to find the true and comparative expansion of any metal, when melted.[1]

The second experiment, was made, to shew a way, how to find the true and comparative expansion of any metal, when melted, and so to compare it both with the expansion of the same metal,

[1] Register, vol. vi, p. 71. It is printed in his *Philosophical Experiments*, p. 94.

when solid, and likewise with the expansion of any other, either fluid or solid body. An accurate account of which is necessary, to compleat a history of expansion or gravitation. The method



of trying it was, by having a vessel full of melted lead, and also a solid body of iron to be sunk into it; this solid piece of iron was about 1½ inch cubical, and into it, was fastened a very small wire of iron, big enough to thrust it under the surface of the melted lead, and make it sink therein, (for, of itself, it swam upon the lead, as wood upon water). This wire was fastened perpendicu-

larly, under a scale, and so much weight put into the scale as served to make it sink under the surface of the lead; then taking it out of the lead, and seeing by the additional weights, put into the other scale, to counterpoise it, first in the air, then in water, or any other liquor, the comparative weight of each of them was easily discoverable.

The reason of the making of which experiment was, to hint the necessity there is, in all experiments fit to be made use of for any philosophical theories, of reducing them to a certainty of quantity; without which, no certain and unquestionable conclusion can be made. Now tho' a certain standard of weight, measure, expansion, power, motion, *&c.* be not made use of; yet if some one determinate measure for each of them be pitched upon, 'twill be enough to make the comparative trials useful; though it were to be wish'd, that some universal, natural standard of measure for all things were found out, those that have hitherto been thought of, having been doubted of, as to their universality and certainty, at all places and in all times.

Pumps and New Windmill.

The experiments appointed for the next meeting were:

1. Comparing of buckets, forcers, and pumps.
2. The model of a new kind of windmill of Mr. HOOKE's invention.

July 11. Mr. HOOKE showed the model of a new sort of windmill.[1]

Dr. HOOK's *contrivance of a very commodious Windmill; communicated to the Royal Society.* [Derham, p. 107.]

July the 11th, I read the preceding discourse and accounts of the two experiments shew'd on *July* the 4th; and further explain'd each of them by verbal discourses. Then I shew'd these two experiments following, which I explain'd by discourses, somewhat in the following manner.

The first, was the module of a windmill, in which were those particulars following considerable, not to be found in any other yet made use of.

[1] An account of it is entered in the Register, vol. vi, p. 73, and printed in Mr. HOOKE's *Philosophical Experiments*, &c., p. 107.

1. That it had no need of any house, but what might be placed, either immediately upon the ground, or under the ground, according to the several uses to which it might be apply'd. Whence follow'd,

2. That the house need not be any impediment to the force of the wind, which it usually is in all other windmills.

3. That it doth of itself turn to all winds, and so needs not the attendance, watching, and labour of men to set it, which is necessary in other mills.

4. That the vanes are contriv'd of the most perfect form, to receive the whole power of the wind, for the cylinder thereof it is exposed to: which is effected by the particular slope of the vanes thereof, whereby the force of the wind becomes equal upon every part of the vane, from the center to the tip, or extremity thereof. An equal progression of wind causing every point of the whole vane to make an equal arch of rotation, or an equal angle at the axis.

5. For that it needeth not so big an axis, nor so strong vanes as other mills, the greatest strength of this being in the way of pulling, the other in the way of thrusting; and this being capable of being strengthen'd by ropes, like the tackling of a ship.

6. For the easy way of producing a circular motion below, without the help of trundles or cog-wheels, which are both a great impediment to its motion, and do wear, and often need repair.

7. For the easy way of communicating a reciprocating perpendicular motion, which is usually perform'd by the help of wheels.

8. For the cheapness of it, there being so many particulars not necessary to this, omitted, which are usually done in other kinds, and not without necessity.

All which particulars consider'd, it makes it to be the most plain, simple, cheap, and easy to be made and used, that has been yet made; and yet the most powerful in its effects, and the most universally applicable to all purposes; (as grinding, bruising, beating, sawing, pumping, placing, twisting, drawing, turning, lifting, *&c.*) that can be made of equal bigness.[1]

[1] I have thought worth while, to insert this account of the windmill

To stop Weights from Falling.

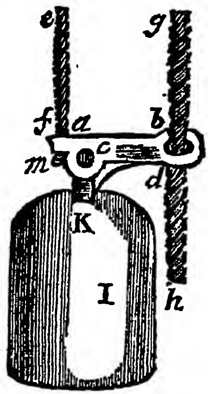
He showed likewise a way to stop any great weight from falling down to the bottom, when the rope or chain, by which it is drawn, chances to break.[1]

July 11.

Dr. Hook's *contrivance to stop great weights falling.*

[Derham, p. 109.]

The second experiment was a very plain and easy way, how to stay a weight from falling, when the rope, or chain, by which it is drawn up or let down, shall chance to break. This was effected



by a small arm extended out from the top of the weight to the side, with a hand, or pipe, at the end thereof, which grasped, or inclosed, another rope or chain, extended from the top to the bottom; which hand, or pipe, was so wide, as to slip freely upon the said rope, so long as the weight was suspended by its own rope; but so soon as that any way fail'd, the hand grasped the side rope fast, and hinder'd the weight from descending to the bottom. This was one of the plainest, easiest, and most simple ways of effecting this end, though the same may be effected divers other ways, as certainly, which I have also contriv'd.

(although scarcely intelligible without figures, or a module, which I never could meet with) because somebody, or other, may be so fortunate to find the module, or, by the hints here given, contrive a windmill like this.

W. Derham.

The figure is probably that printed on page 584.

[1] Register, vol. vi, p. 75. It is printed in his *Philosophical Experiments*, p. 109.

The explicating it, by a scheme, makes it the more intelligible. I represents the weight, *a b* the arm, moving with a joint at *c*, upon the other part of it *k*, fast into the weight, *e f* represents the rope, by which the weight is either drawn up or let down, fasten'd to the elbow *m*; by which means the wrist, and hand of the arm, is kept at right angles with the part fast in the weight, and so the hand slips freely upon the greater rope *g h*, extended from the top to the bottom, to which the weight can descend; *d* represents a spring, by which, so soon as the rope of the weight, which holds by the elbow *m*, fails, the arm is extended streight; by which the hand *b*, presently holds fast the rope, or chain *g h*, by being made oblique to the perpendicular, and, so creeking the rope, and so hinders it from falling; as, by the experiment shewn, plainly appear'd.

The use of which contrivance, though possibly it might, to some, seem very trivial and insignificant, as seeming to be calculated for keeping a clock, or chime weight, from falling, is not altogether so slight and foolish; for even for that use it may sometime or other possibly save 100 pound expence, and the lives of some men. But if apply'd, in general, for the hindering weights to fall, it may deserve a somewhat better value, and be found very considerable, since it may be very instrumental to save many mens lives, and much charge, and great inconveniences, which do very often now, for the want thereof, happen. For whereas, in many mines, the men themselves are often drawn up and let down in buckets; and generally the ores, stones, waters, and divers other things, belonging to those works necessary for procuring ores, or other minerals, are so conveyed; and upon the failing of the rope, chain, or other part of the engine, do often fall from top to bottom, and so are not only dashed in pieces themselves, but destroy, and do oftentimes irreparable injury to men, or what else they meet with in their fall. By this means, all such bodies are secured from the fall, and kept hanging at the place where they were when the rope brake, or other part of the engine fail'd, and thereby the bodies themselves are preserv'd intire, and no other harm done by their fall. The same thing is applicable also to men, ascending, or descending, by ropes or rope-ladders, and to stones, timber, or materials for a high building.

July 18. *Experiments and Devices shown to the Royal Society.*
[R. S. MS. No. 65.]

Chariots.

The experiments appointed to be made by him at the next meeting were about chariots.

Hooke's Joint.

[*July* 18.] Mr. HOOKE showed two experiments for the converting an horizontal circular motion into a perpendicular, or any otherwise sloped circular motion, or *vice versa*, &c. of which he gave in the following account.

Then I showed the two following experiments or inventions, which were both to do the same thing, but by different ways, and with different advantages; namely, *how to contract an horizontal circular motion into a perpendicular, or any otherwise sloped circular motion, and vice versa*, keeping in all parts of the revolution the same velocities with the velocities of the corresponding parts of the revolution of the circulating body, that communicated the motion. And this without wheel pinions or trundles, and without any considerable friction or impediment to the motion of either; which are of great use in mechanic inventions and engines, though possibly not so well understood by the greatest part of mechanicians, much less by those, that make use of them, and least of all by others, not concerned in either.

The first was performed by the means of a double cross, after the same manner contrived with the single cross described and explained in an invention I formerly showed to this society, of an engine to make all manner of dials, both mechanically and geometrically true to the minutest divisions, without the help of calculations or troublesome * * * * * of practical geometry.

The only thing to be taken heed of in this invention was, that the arms of the intermediate piece between the horizontal and perpendicular axis should be in the same plane, and that the axis of it should lie equally inclined to both the other axes.

This way performed the effect with the greatest steadiness, freeness, and easiness of motion, without friction or wearing, and so for most uses is best, though it be somewhat more troublesome and chargeable in the making; but for divers other uses the second way was more easy, which was done only by a ring joining the two ends of the cylinders (namely, of the horizontal and of the perpendicular) together, each of which ends had a hole or loop through it fit to receive the said ring, as by the model and experiment made therewith plainly appeared.

This second or latter way, though it were more simple and easy to make, was not in the use thereof, where strength was

very considerable, or equality of motion was necessary, so good as the former; though for many ordinary uses it might serve well enough, and so be of more general and common use than the other, and so is valuable upon the account of its plainness and practicableness. For in mechanics (contrary to the opinion and practice of most projecting mechanics and ignorant spectators) an invention is valuable not for the clutter, pomp, complication, and difficulty; but for the simplicity, plainness, obviousness, and easiness, both of understanding, making, using, and repairing, which makes it approach the nearer to the example of nature, which doth nothing in vain, or by longer and more difficult ways, which may be done by a shorter.

Nor were these inventions only useful for communicating a motion of rotation true at right angles, but of doing the same thing at any other inclination of the axes, which in many cases is very useful; as also for conveying a vibrating or reciprocating motion, and further likewise for conveying a reversed, circular, or reciprocated motion to a parallel cylinder, some good uses whereof I may hereafter make appear.

Sept. 22. To present Capt. KNOX a perspective. A picture box an azimuth perspective. A Longitude Clock. Mr. FRAZERS Dipping Coyne in Glue. A tangent projection from a projection on a sphere, but only great circles which pass the center will be straight lines, the others will be conicall curves.

[Hooke's *Diary*. MS. *Sloane*, 1039.]

Hautefeuille on Object Glasses.

Oct. 24. With M. JUSTEL'S letter came a book presented to the society by the author, Monsieur HAUTEFEUILLE, about the improvement of perspective glasses, and the best ways of finding the focus of long objectives; concerning which book the society's opinion was desired by the author.

Mr. HOOKE having just cast his eye upon it did not seem to find anything new, which might be of very great concern; but the book being longer than he could read at present, it was referred to be perused by some of the members.

Mr. HOOKE said that he had formerly contrived glasses of parallel pieces, but found them unserviceable.

Mr. HOOKE was desired to bring in such experiments as he should have ready at the next meeting.

Strength of Wind.

Oct. 31. Mr. HOOKE brought in an experiment, in order to measure the strength of the wind.

Oct. 31.

Dr. Hook's *way to take the impressions of medals, &c. imparted to the Royal Society.* [Derham, p. 111.]

Having been shewn, by Mr. *Frazier*, the impressions of several of the King of *France's* medals, in a certain thin transparent substance, much like *Muscovy* glass, but much more tough; on which, on the one side, appear'd the perfect impression of the medal, in *Entaglio*, or sunk in; and, on the opposite side, the very figure of the said medal in *basso relievo*, or swelling out. And, considering what way this might be done, having formerly taken off the figure of certain carvings, by glue, so as to be able to cast them in plaister of *Paris*, or burnt alabaster; upon making trial with a glue made of *icthuocolla*, dissolv'd over a gentle heat, in coarse spirit of wine, by laying it upon a fair stamp'd crown piece, and suffering it to lie a considerable time, till it was thorough dry, cold, and hard; I found that it afforded me the same kind of substance, both for toughness, transparency, and fitness, to receive and retain the impression of the coin upon which it was laid, as the substance shew'd me, containing the impression of the *French* medal. This I shew'd the Society, and explain'd to them the way of doing it. And also related, that the same impressions might be so taken with common joyners glue; but the plate would not be so tough, nor so transparent.

The President mention'd, that there had been a certain *Frenchman* here in *England*, some time since, who had certain transparent plates like *Muscovy* glass; with which, he could easily copy out any picture or print, by laying it upon the same, and writing upon it with ink, as on paper; the same being very transparent; and so causing the print, on which it was laid to appear very plain through it: and inquiring, whether I could do the same, upon my affirming that I could, he desir'd that I would shew the experiment of it at the next meeting.

N.B. Dr. Lister *mention'd the way of contracting seals with mouth-glue.* (See p. 624.)

John Davis's Inventions.

Nov. 7. Mr. Aston brought in a proposal of Mr. John Davis of Nottinghamshire, of several things performable by himself; as to make a machine to weave loop lace; an engine for the

making of twist; a way to card wool with a wheel; a way to make a jack go longer than ordinary without winding up or pulleys; a pattern of a machine for weaving, as in a silk stocking frame; some ways supposed by him new, of raising water, &c.

Mr. Hooke conceived, that most of the things, which he proposed, were already practised in London.

To take Casts of Medals.

Mr. Hooke showed the impression of a medal taken off upon fish-glue or isinglass; which he said was done by dissolving the glue in spirit of wine, and laying it upon the medal till it be dry.[1]

Nov. 7.

Dr. Hook *imparted to the Royal Society this Preparation, to copy any Picture, &c.*

I produced a plate, made according to the preceding desire; which had the same properties with that which was made by the *French* gentleman. This was very thin, and as transparent as *Muscovy* glass, or *Selinitis*. It was also tough, and would bear ink as well as any paper, and so was fit to make use of, for any experiments for drawing, or copying pictures or maps. The manner of making it, I explain'd to the Society, to be thus. First, I prepar'd a very thick cise of *icthuoceolla*, well dissolv'd in spirit of wine, and then clear'd from all its rags and foulness, by straining it through a clean cloth; then taking a looking-glass plate, well smooth'd and polish'd, I rubbed the same all over with a fine rag, moistened a little with pure sallad oil; but so as only to hinder the substance that was to be pour'd on it from sticking to it, but not to make it foul or uneven. Having so prepar'd these things, I heated the sise, and, when again pretty cold, I pour'd it upon the oiled side of the glass plate, and so taking the plate, and inclining it this way or that way, till the whole plate was cover'd by the sise, I laid the plate horizontal, and suffer'd it to lie so till it was thoroughly dry.

Merry's Mathematical Papers.

Nov. 14. Mr. Hooke was desired to go with Mr. Aubrey to Mrs. Merry, to examine the mathematical papers of her husband.

[1] His account of this method is entered in the Register, vol. vi, pp. 81, 82, and printed in his *Philosophical Experiments and Observations*, pp. 111, 112.

Anemometer.

Mr. HOOKE showed an instrument to measure the velocity of the air or wind, as follows:

I showed *an instrument to measure the velocity of the air or wind, and showed to find the strength thereof,* which was by four vanes put upon an axis, and made very light and easy for motion; and the vanes so contrived, as that they could be set to what slope should be desired.

It was several times tried and examined in the long gallery of Gresham College, whereby it appeared, that by walking from one end thereof to the other, and carrying the same above one's head, the doors and windows of the said gallery being shut, and so the air within it being not in motion but stagnant, the instrument made so many turns, as there were circumferential lengths of the said vanes in the length of the gallery: and if by trial it were found to be more or less than the due measure of those circumferential lengths, then by setting the said vanes either flatter or sharper, in respect of the way of its motion through and against the air, the same was easy to be adjusted.

The use of which may be of very great consequence in the business of sailing and steering a ship upon the sea, and for examining the power and strength of the wind upon land in order to the theory of shipping, for which it was designed.

Copying by Fishglue.

Mr. HOOKE showed a convenient way of copying anything, &c. by making a thin plate of fish-glue dissolved in spirit of wine, and well clarified; then poured upon a glass plate as thin as paper, lying there till it be dry; at which time the lines of the object may be drawn upon it as they appear under.[1]

Hooke's Lodgings and Service.

Nov. 21. Meeting of the council in Mr. HOOKE's lodgings.

Nov. 24. At a meeting of the council at Mr. HOOKE's lodgings, It was resolved, that it is the opinion of the council, that Mr. HOOKE should be owned and assisted by the society as far as lawfully they may, and is warrantable by the orders and Journals of the society.

It was ordered that Mr. HOOKE have £15 upon account, as part of what he is to have, when he brings in the account of the experiments made this year: And

That inquiry be made for such a clerk for the society, as may perform the laborious part of the Secretaries office.

[1] Register, vol. vi, p. 82, and Dr. HOOKE's *Philosophical Experiments*, p. 112.

Report on Experiments.

Nov. 27. Mr. HOOKE acquainted the council, that he intended to write an historical account of the experiments, which he had showed before the society, together with a declaration of the use and consectaries of each, and an idea of natural philosophy built upon them: and he desired, that the experiments not heretofore clearly made, and which were imperfectly entered, might be repeated at the society's charge: which was ordered accordingly, with this addition, that at the meeting before any such experiment made, he should give the society notice, that such as pleased might be present.

Ship's Patent Log.

Nov. 28. Mr. HOOKE showed an instrument, which was one part of a waywiser; his account of which was as follows:

I showed an instrument I had contrived, and showed some of the society about twenty years since, by which the way of a ship through the sea might be exactly measured, as also the velocity of any running water or river, and thereby the comparative velocity of it in its several parts. By this also the quantity of the water vented by any river into the sea, or any other river, might be found. It was one part of a waywiser for the sea; the whole engine being designed to keep a true account, not only of the length of the run of the ship through the water, but the true rumb or leeward way, together with all the jackings and workings of the ship. This part of the engine now shown was the vane, fly, or first mover of the whole, feeling as it were, and distinguishing the several qualifications of the ship's course; but was to be regulated by several other additions in the completed engine, which I design shortly to get executed.

Dec. 3. *Discourse on the utility of the Suns rays* let into a dark room. [Printed in *Posthumous Works*, p. 473.]

n. d. Discourse on a *Universal Standard for Measurement.* [*Posthumous Works*, p. 472.]

Steelyard.

Dec. 5. Mr. HOOKE brought in a model of a balance for finding any desired part of a weight given. The beam was suspended, and poised in a part, from which one arm was ten times as long as the other. The scales also were one to the other as ten to one.[1]

Dr. HOOK'S *several discourses of improvements of Scales, Beams,*

[1] It is entered in the Register, vol. vi, p. 85, and printed in his *Philosophical Experiments*, &c., p. 113.

and other instruments, for weighing bodies more nicely; and first, one to find any desired part of a weight, or body to be weigh'd.
[Derham, p. 113.]

I produced *an instrument for the speedy and exact finding any desir'd part of any weight given, whether commensurate, or incommensurate.* The instrument, (being only a module, and to serve only for explication and experiment, and not for constant and continual use) was a slender fishing-cane, streightened very well, of about 4 foot in length, and tapering from one end to the other; this material I made use of upon a double account; *first,* for its stiffness; and, *secondly,* for its lightness, that I might, as near as possible, make it to be without weight, and bending, and so approach to, or represent, a *mathematical line.* Now the part, I propos'd to find, being a *decimal, centesimal, millesimal,* or the powers of the *decimal fractions,* I divided the cane into eleven equal parts; at one of which, from the greater end, I, with a needle, drew through it a small silk thread, by which I suspended it; and by adding lead to the shorter end, I pois'd it, until it came to an *equilibrium,* and so it hung *horizontally.* Then I made two *scales,* with two rings, whose inner edges were thin and sharp, by which they might hang upon the ends of the *horizontal,* or *equilibrated* cane. The *scale* and *ring,* for the *greater* and *shorter end,* was made ten times as heavy as the other *scale* and *ring* for the smaller and longer end. These being thus prepar'd, I hung on the *scale* upon the *greater* or *shorter* ends, at any distance from the thread: then, hanging on the little scale, upon the lesser end, moving it nearer and farther from the suspending string, till the beam hung *in equilibrio*; the which became an instrument for finding the decimal, centesimal, or millessimal parts, or fractions of any weight given. Suppose a pound be to be so divided; put the pound into the great scale, and then counterpoise it with weight, as of sand, water, minium, *&c.* in the lesser scale; this shall be a tenth part of a pound: remove the pound, and put the decimal counterpoise in the greater scale, then counterpoise this in the lesser, and this shall give a centesme of a pound: remove the decimal, and put the centesme in the greater, and the counterpoise to it in the less, shall give the millesimal part of a pound, and so onward for the ten thousanth,

hundred thousandth, or thousand thousandth part of a pound; which, this way, may be most exactly found and determin'd: And the like for any other assignable part whatsoever of commensurate, or incommensurate proportion, to the whole quantity, of what weight soever; the beams being accordingly proportion'd in strength and dimensions, whether it be for great and massy bodies, or exceeding minute and curious; and, by this means, with some small addition, the smallest bodies may be as certainly weigh'd, as the most tractable, even to the thousand thousandth part of a grain, far beyond the reach of the hand, or the naked eye. And, as the *microscope* doth help the *eye* to make *invisible bodies*, and *parts visible*, so may this help the hand to make the *intractable bodies tractable* and *ponderable*, and *comparable*, by other trutinations than those of sight; which is of considerable advantage in the inquiry after the several natures of the *intims of things*, as I may hereafter shew, more particularly. In the mean time, I conceive, there was no great reason for any, either to affirm the experiment false or erroneous, or to slight it for its plainness and obviousness; since any, that understands *mechanick principles*, will save me the labour of making a demonstration. And how obvious soever it be now known, yet I do not find it hath been taken notice of by any writer of *mechanicks*; nor did I ever know any that had used it, or taken notice of it, for this purpose; and though it may be said to be a *stilyard*, yet 'tis as differing from the common use of the *stilyard*, as that is from a *common beam*. I mention'd also, how necessary an *instrument* this was in almost all *Philosophical Examinations*, especially in all trials that concern the limits and bounds of powers, in the *intims of bodies*. This *proportional balance*, will be of general use, and to such, particularly where weights are troublesome to carry and remove; and, I suppose, the only reason, why it has not been used, is, because it has not been thought of; though it were altogether as obvious, as to set an egg on end.

This instrument being easily understood without a figure, I have therefore omitted the giving any.

A second instrument for weighing; or, a sort of essay-scale.

December 12, 1683, I produced another experiment, which was also an instrument for weighing, which might also be of very

general use; and that was not only for examining the weight of any sort of gold or silver coin, or any other vessels or pieces of those metals: but also for examining and essaying the nature of the metal itself, of which those pieces, or vessels, should be made, both as to the species of the metal, and also as to fineness, purity, or the contrary qualifications of them. Now though this be to be done by means of ordinary gold scales and weights; yet, I dare affirm this way to be altogether as sure as the other, and abundantly more easy, both for carriage and use. And there might as well have been objections made against the art of printing, because a writer was able, before that art was found, to have wrote letters, and words, as fair as they could, by that art, be printed. The invention of the instrument was grounded upon the theory of the nature of springs, which I have formerly shew'd, and explain'd in this place; and the way of examining the goodness or badness, of this kind of metal, and of discovering the species of the metal itself, was grounded upon the experiment of *Archimedes*, improv'd and explain'd by *Getaldus*; which two theories, being rightly understood, will take off all objections against the truth and reality thereof, with all impartial persons.

The instrument was made of a coyle of brass wire, one end of which, was held in the hand; and, to the other end, was fasten'd a small net of hair, in which net, the piece of metal to be examined was put; and then the whole was lifted up by the hand, and, by means of a small top of a feather, fasten'd to the lower part of the wire, the length of the whole spring augmented by the weight of the piece try'd, was observ'd, and by the division on the said feather, the number of grains were to be taken notice of; this gave the quantity or weight of the piece itself in grains. Then, for the second qualification of the said metals, it was to be found by holding the piece (now weigh'd, and in the scales made of a net of very fine hair) into fair and clear water, and observing by the relaxing of the spring, how much the piece grew lighter; for thereby the specifick gravity of the metal itself, compar'd to that of water, was exhibited; and this without making use of differing, or indeed any weight at all.

Account of Experiments.

Dec. 12. Mr. HOOKE being sent for in, and required to deliver

to the Secretaries the written account of the experiments made by him as curator during the last half-year, he promised to deliver the said account to Mr. ASTON by Christmas Day.

The council recommended to Mr. HOOKE the retrieving, and delivering to be entered, such other experiments, as had been made by him as curator during the last seven years.

Mr. HOOKE was desired, when he should make any experiment at the society's charge, to acquaint them with it the week before, and to have their consents; some persons, if it should be thought necessary, being appointed to be present at the making of the experiment.

Petrifaction.

Mr. HOOKE mentioned a place, where shells falling upon a shore were petrified, some more some less, as they lay more or longer in the water.

Mr. HOOKE having mentioned several sorts of petrified oysters, Dr. LISTER seemed to allow but two sorts of oysters in Europe, with either of which the rock oyster-shells had no similitude, as having no striae on the outside going from the valve to the rim.

Loadstone.

Mr. HOOKE proposed the trial, whether grinding a loadstone would destroy its polarity; though it were true, that filing would confound it.

It was also queried whether the touching of a needle penetrates it, and gives a virtue to the inner part of the iron.

Steelyard.

Mr. HOOKE read a paper concerning the use and convenience of the beam or steelyard proposed at the last meeting.[1]

He also promised, that such a beam should be made, and showed the society at their next meeting.

Silver Ore.

Mr. HOOKE showed a sort of glistering ore, which, Dr. LISTER said, was called *throstle-breast;* and that it yielded its silver easily, but that the quantity was not great.

Specific Gravity Method.

Mr. HOOKE showed a way to weigh gold, &c. and at the same time to examine the bulk. The body to be weighed in water was hung from an helical coil of wire. The weight was to be judged from a body placed within the wire, and divided conveniently.

[1] This paper is entered in the Register, vol. vi, p. 88, and printed in his *Philosophical Experiments*, &c., p. 116.

The trial hereof was not made, the apparatus being only contrived to show the manner how the thing was to be done.

Steelyard.

Dec. 19. Mr. HOOKE produced a steelyard, whose beam was to a third proportion; whereby he showed the conveniency of weighing of bodies with a less number of weights, than usually is done any other way, and declared, that he designed no more by this steelyard than the giving of an aliquot part of any assigned weight; so that for any different division of weight there must be a different steelyard. His account of it was as follows:

I showed a pair of *scales and weights* made by order of the society. The beam made *for triplicating or tertiating any weight small enough to be weighed by it.* And the use of it was for accommodating our European weights to those of the Indies. So that with a pile of weights of 21⅓ pounds troy weight, any body to 64 pounds troy could be exactly weighed. So that almost a single cundarine would turn the scale, when charged with that weight. Which cundarine is but the thousandth part of our pound troy.

These sort of scales will be of excellent use for weighing of great weights, because the beam may with ease be so made, as that one-twentieth part of the weights may serve, and the beam and triangle need not be much more than of half the strength and weight, and yet the beam more nice and curious than the best made the ordinary way. Nor is this at all the same with the butcher's steelyard, this having scales as ordinary beams, and as great variety of weights, though each of them be but one-twentieth part of the weights necessary to be used in the common way. And whereas in a ton weight there are not less than forty half-hundredweights necessary sometimes to be removed from place to place; in this way one pile of weights, amounting in all to one hundredweight, will fully and more exactly perform it; and any weight may be weighed exactly thereby, that is not less than a scruple, nor bigger or heavier than a ton, which I think is not to be done by any beam yet used.

Specific Gravity Method.

Mr. HOOKE read a paper concerning his way of weighing bodies by an helical coil, whereby not only the quantity of the weight of any body should be discovered, but likewise the quantities of it; as in the mixture of any metal, &c. which, he said, could not be done so commodiously by any other instrument.

Dec. 26. Letter from HE. OSBORN to HOOKE.

1683/4

Stone of Pyramids.

Jan. 9. Mr. HOOKE thought, that the pyramids might be built upon a rock of such stone in the place where they stand.

This discourse giving occasion to mention Mr. VERNON, Mr. HOOKE was desired to return the copy of Mr. VERNON's papers.

Balance.

Mr. HOOKE showed the contrivance of a kind of balance, which would give any desired part of a weight.[1]

The charges of one of these balances being but small, he was desired to get one well made by a good workman.

Jan. 9.

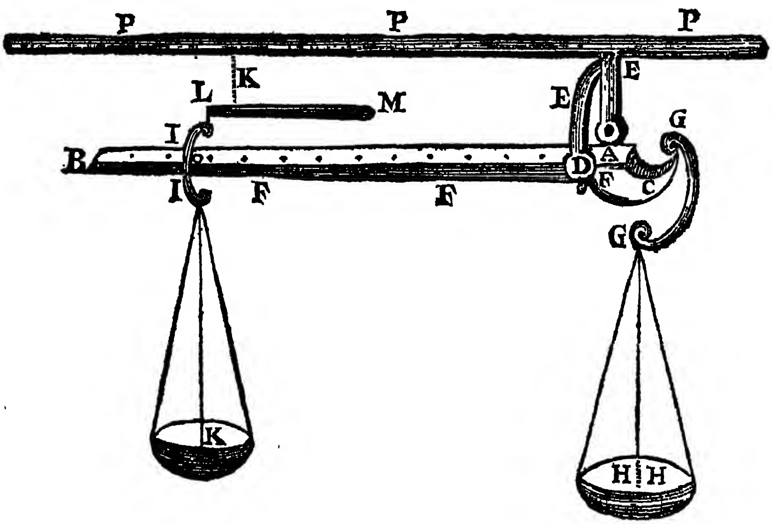
A third instrument for the same purpose.

Scales copied from the *Royal Society*, Regist. Numb. VI. p. 136.

I shew'd a module of a *beam, whereby readily to find any aliquot, or aliquant part of any weight given.* The beam was made in the same manner as the first that was shewn; namely, that with a cane; but whereas that was only then divided and design'd for decimation, or decuplation, the longer end of this was divided into 12 equal parts, and the face of the beam was made so wide, as to be capable of admitting subdivision by diagonals. The shorter end was one twelfth part of the longer; at which distance, the great scale was properly fixed, wherein the weight, to be subdivided, was to be put: this scale, when empty, counterpoised the longer end, without any scale suspended on it: and that the removing of a scale might make no alteration of the former *equilibrium*, the weight of the same was wholly taken off by a proper counterpoise, so that the scale had no weight at all upon the beam. The way of finding any desirable part of a weight given, was thus: if the part were not smaller than a twelfth part, then the same might be easily found by one operation, by placing the scale at such a distance from the axis of the beam, on the longer end, that the same shall be in such proportion to the shorter end, as the whole weight is to the part design'd; for instance, having a lump of *ambergrease*, of an unknown weight, but 'tis to be divided into three shares, which are to be in proportion, one to

[1] It is entered in the Register, vol. vi, p. 134, and printed in his *Philosophical Experiments and Observations*, p. 118.

another, as 345, 234, and 123, to find each of these, I thus proceed; adding all the proportions together, I find they make 702; then, by a sector, by the line of lines, I open the compasses to the length of the shortest shank of the beam; and, by that, open the sector to 345; then, on the same sector so opened, I open the compasses to 702, and set off that distance on the longer shank of the beam, and there place the lesser scale; then putting in the lump into



the greater scale, I counterpoise it in the less, and that gives me the first share, which is as 345 to 234, and 123; this weight I lay by.

Then upon the same opening of the sector, I take off 234, and setting it on the longer shank, I place the lesser scale, and proceed as before; and this gives me the weight of the second part, namely, 234. Then the difference between the sum of these two, and the whole, in a common balance, gives me the third, *viz.* 123.

If the part, to be found, be less than a twelfth part, and not less than a one hundred forty fourth part, by some previous division of it, by once weighing, I reduce it to such a part, as, by the second weighing, I find the part, to be found, will not be less than a twelfth; and then I proceed as before. This may be perform'd, either by finding two dividers of the part, both which shall fall within the compass of 12; or, if it be a prime number,

then by extracting the root of it; which may be done arithmetically in decimals, to what accurateness shall be desir'd, or by a line of superficies on a sector, or by a table of logarithms.

If the part to be found, be less than a one hundred forty fourth part, and not less than a seventeen hundred twenty eighth part, then it must be perform'd by three dividers, if such can be found, that will fall to be each not less than a 12th, or else, by the extraction of the cubick root. If the part be less than 1728th, and not less than a 20736th part; then, by finding four dividers, each, within the compass of a twelfth, or by extracting the quadrato, quadratick root, the part may be obtain'd by four operations.

Jan. 16. Mr. HOOKE being sent to, but being absent, Mr. MEREDITH was desired to speak to him to deliver to the Secretary the particulars of his experiments.

Weighing.

Mr. HOOKE showed a way how to give the proportions of two weights one to another, the apparatus being not designed for an accurate trial.[1]

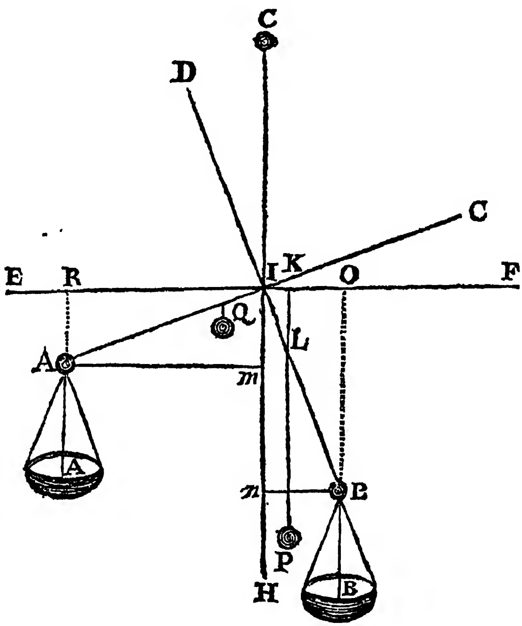
Jan. 16.

The fourth instrument for weighing.

I shew'd a new *instrument I had invented, by which, immediately, and without any trouble, the comparative weights of any two bodies given, might be found; if, at least, the beam were of bigness enough to bear them.* The beam was made in the form of a cross, equilibrated upon a sharp edge in the center; the scales were hung upon two ends (not opposite, but) next together, which were also equilibrated; the smallest weight, in either of the scales, would make the arm, by which it hung, to stand perpendicular, and, consequently, the arm that bore the other scale, to lie horizontal. The bodies to be weigh'd, were each of them put into the scales, one in the one, and the other in the other; and so suffer'd to take their posture (which they would presently do) by putting the beam in such a posture, that the distances of their points of bearing, from the perpendicular under the center, would be in reciprocal proportion to their weight. Dividing then the arm,

[1] His account of this instrument is entered in the Register, vol. vi, p. 138, and printed in his *Philosophical Experiments*, &c., p. 121.

on which the greater weight hung, into ten equal parts, and each of those into ten, and, if the beam will bear it, each of those again into ten, all of which, will make one thousand equal parts, I place three pins upon each of the other arms, which cross the aforesaid arm at right angles; the first two, at the extremities, the next two, at the distance of one tenth from the center, and the third pair, at the distance of one hundredth; then I provide



two bullets, equiponderant to each other when fitted, the one with a small clew, somewhat more than the length of the longest diagonal of the two suspending arms, with a ring at the end to hang upon one of the pins, the other, with a ring only. Then, according to the difference of the bodies counterpoising each other, I hang on the plummet and line upon that pin of the arm over the heavier body, and is nearest to the extremity; from which the plumb line may fall upon the divisions of the arm, and counterpoise it also with the ring and bullet hung upon the corresponding pin on the opposite arm, then shall the plumb line shew, upon the divisions, the proportionate weight of those two

bodies. I need not shew the great use and benefit there may be made of this beam in all philosophical inquiries, since they are obvious enough.

Let A C, B D, represent the cross beam, moving on I, the scales hanging at A and B. The weights being put, the heavier in B, the lighter in A, the cross positeth itself as in the scheme in respect of the horizontal line E F, and the perpendicular G H; and their comparative weight is found by their several distances from the perpendicular I H, that is, as B N to A M, so the weight at A, to the weight at B. Thus far is clear from the principle of staticks. Let K P represent the plumb line, suspended at K; I say then, that I K, is to I L, as B N is to A M, or, as the lesser to the bigger weight; for A M, is equal to I N, and the angle N I L, is equal to I L K, therefore K I L, is similar to B N I, therefore as K I to I L, so B N to N I = to A M, so the weight at A to the weight at B. Q.E.D.

Under-currents.

Jan. 23. Upon occasion of under-currents, Mr. Hooke mentioned an Italian book, lately written to this purpose about the Bosphorus of Thrace. Dr. Aglionby was desired to take the trouble of perusing the book.

Hammering a Magnet.

Mr. Hooke remarked, that by striking a needle with a brass hammer, the pole might be changed from north to south. To which it was answered by Dr. Wallis, that there was nothing of hammering mentioned in this relation, but with more probability a new touch of a magnet.

However, Mr. Hooke was ordered to show at the next meeting, how the pole of a needle is altered by striking, that the applicableness to this case might the better appear.

Sapphire Electrical.

Mr. Hooke said, that he had found a sapphire electrical. though denied to be so by Dr. Plot.

Balance and Japan Scales.

Mr. Hooke showed the manner of a balance, in which the weight was not mixed, and which had a greater latitude than the common ones: and he showed a pair of Japan scales;[1] which

[1] His account of the balance and Japan scales is entered in the Register, vol. vi, p. 142, and printed in his *Philosophical Experiments*, &c., p. 123.

appeared to be not at all exact, and was said by Dr. WALLIS to have the point of suspension a great deal above the centre of gravity.

Jan. 23, 1683/4.

The description of a pair of Japan scales and a Japan stilyard.

I produced, and shew'd three several kinds of beams, for weighing the gravity of bodies; the first, was a pair of *Japan* scales and weights, made and adjusted in that country, and that with very great care and curiosity. The beam was made of a round rod of brass, tapering a little from the middle towards the ends; which were flattened perpendicularly, and had each of them a small hole drill'd through it, tapering both ways to the middle, leaving an edge round the middle of the hole, through each of these holes, was put a brass ring of wire, by which the dishes were suspended by four strings. The cock, or tongue of the beam, was neatly soder'd into the middle of the beam, about two inches broad below, and ¼ of an inch at the top; and about the middle, between the beam and the top, was put the pin, upon which the beam play'd; the handle of the beam was also made of a kind of ring of brass, and the lower part thereof, was slit so as to receive the cock, that it might just freely move between its sides and no more; and the pin rested upon two holes made in the sides of the said handle; the top of this handle had a small tongue of brass, of the same breadth with the top of the cock of the beam, and pointing so directly at it, when in *equilibrio*, and so near approaching it, as just not to touch it. This beam was suspended by a convenient frame of wood, as to hold it steady whilst it was made use of; and to find exactly the *equilibrium*, by giving a little knock with a small wooden mall, upon the handle, there was caused such a shaking, as made every thing settle into its due place; and, by the ends of the two opposing cocks, or tongues, the agreement, or difference, was discoverable. The weights were all curiously, and very exactly, made of brass; which, that they might not be adulterated, were, all over the surface of it, stamped with the Emperor's Seal, and the quality of each engraven upon it in the *Chinese*, or *Japanish* characters. These are, by a severe penalty, prohibited to be exported into any other place, and are of great value in the country itself. The weights are *Cunderines*,

Maces, and *Tales*; ten *Cunderines*, making a *Mace*; ten *Mace*, a *Tale*; and 10 *Tale*, one pound *Troy*.

The second, was a *Japan* stilyard, made upon the same principle as our common stilyards, but with greater curiosity, and for smaller weights, than we generally use them, serving to weigh any weight from a *Cunderine*, to two pound *Troy*, or twenty *Tale*. The beam was made of a tapering rod of ivory; the scale, or dish, at the greater end, was hung by a strong thread of silk, which pass'd through a hole in the bigger end of it; instead of handles also, there were three strong threads of silk, at several distances from the former, which pass'd through three several holes in the beam; and to each of those three handling threads, was adjusted a line of divisions upon the sides of the tapering longer arm; the weight was of brass, and suspended on the longer arm, by a small bow of silk, which might be easily slipped to and fro, as occasion required. The whole instrument was very compleat, and nice enough for the purposes it was design'd for, to wit, for weighing silver and gold, *&c.* in the way of trade.

The third, was a stilyard of my own invention, by which the weight of any body, that could be weigh'd in it, might be found without the trouble of removing the weight, as in the common stilyard; and, by means of a plumb line, after the manner of the balance I shew'd *January* the 16th, the particular weight of whatever was put in the scale, was presently manifested; and it had this great conveniency in it, that the divisions, by which the weights were determin'd, were altogether as great at last, as at first, being all equal. The conveniences and uses, are obvious enough in the weighing, either of great or small bodies, all being to be perform'd with great speed, and as great exactness, and with much less trouble.

[DERHAM here draws attention to the Minutes of the Royal Society of Octob. 25, 1677, describing an experiment made by Dr. HOOKE. See p. 445.]

Drilling and Magnetism.

Feb. 6. Mr. HOOKE being called upon for the changing magnetical poles produced a drill,[1] whereby the intention of the

[1] His account of this is entered in the Register, vol. vi, p. 145, and printed in his *Philosophical Experiments and Observations*, p. 128.

experiment appearing, it was not proceeded on; especially as he declared, that it related not to the instances of the magneticalness of lightning mentioned at the last meeting.

Feb. 6.

Dr. HOOK'S *experiment before the Royal Society concerning Magnetism in Drills, &c.* [Derham, p. 128.]

I then produced the *apparatus* for the experiment appointed me last meeting, in order to make out my assertion, that the magnetical virtue in steel might be excited, and considerably increased by a body not generally accounted magnetical; and therefore, that the affirming a body to be magnetical, because it excited that virtue would not always hold good. The experiment I made, to examine this opinion, was this. I took a drill made of steel; and, lest it should have had any determinate virtue in it, as to polarity, I heated it red hot in the fire, and so suffer'd it to cool, quenching only the very drilling point of it in cold water: when it was perfectly cool, I apply'd a needle to it, and found, that which end soever I turn'd downward, it would attract the south end of the needle, and the upper end would attract the north; and this, as often as I repeated the turning of the drill, and apply'd the needle to the ends of it. So that it plainly appear'd to have no determinate polarity at all, as a drill, or the like piece of steel, touch'd by the loadstone. Then I caused a piece of brass to be put upon a table, and holding the drill very near with the same inclination, and in the same line, that a dipping needle left free, when well poised, would situate itself; I caused the drill to be mov'd with a drill bow, so as to drill a pretty deep hole in the said piece of brass, and thereby to warm or heat the top thereof. Then, examining it again with the needle, as I had done before, I found that the drill by this boring, or agitation, had acquired a polarity or directive virtue, as well as an attractive for the point of the drill, which, in drilling, respected the north, whether it were held downwards or upwards, always attracted the south end of the needle; and the contrary end in like manner, in either posture, attracted the north, in the same manner as if the point thereof had been really touched with the needle. In the like manner, I found by trying with a steel chizzel by striking of its end, when placed in the proper position

of the dipping needle, that much the same effects would be produced.

Hereupon it was objected, that brass itself was a magnetical body, and therefore that this was not a sufficient eviction; whereunto I replied, that I conceiv'd any other hard body, placed instead of the brass, would produce much the same effect.

I did therefore propound to have the same tried with hard wood, ivory, bone, glass, or stone, which have not hitherto been accounted magnetical bodies, to see whether they would not be a means of exciting this magnetical virtue; for if so, then either all bodies, that are hard, must be said to be magnetical, or else it will not necessarily follow, that every body that excites this virtue, is therefore to be esteem'd magnetical. And this the rather, because as I have, in part, shewn in this place, and as I shall hereafter make out more at large, there may be produced in other bodies, as well as steel, iron, or the like, a quality much resembling that of the magnetical; wherein, notwithstanding, neither the magnet, steel, iron, or the magnetical virtue, or power of the earth, is any way concerned.

Expansion of Water in Glass.

Dr. CROONE said, that he observed water, which he had put into a bolt-head, to rise higher before there was anything of freezing in it.

Mr. HOOKE attributed the rising of the water in the neck of the bolt-head to the shrinking of the glass.

Strength of Ice.

Mr. HOOKE gave an account, that himself, Dr. AGLIONBY, and Mr. MEREDITH had made a trial of the strength of ice:[1] and that a piece of ice $3\frac{1}{4}$ inches thick, 4 inches broad, and having a foot clear between the places of bearing an iron rod in the middle, bore 300 lb. weight very well, but 350 being laid on, it broke after some time, this weight with the two beams making near 400 pounds.

Dr. HOOK'S *experiment about the strength of ice.*

[Derham, p. 130.]

Next, I gave an account of an experiment, which I had caused

[1] This account is entered in the Register, vol. vi, p. 146, and printed in Mr. HOOKE'S *Philosophical Experiments and Observations*, p. 130.

to be tried in the presence of Mr. *Meredith*, and Dr. *Aglionby*, of a piece of ice, plain'd true square, of about fifteen inches in length, four inches broad, and 3½ inches thick; this was pretty solid, having no more blebs in it than common ice usually hath. This piece of ice, so squar'd, was plac'd upon the engine made on purpose for examining the strength of bodies, as to bearing. The places, whereon the two ends rested, were just twelve inches asunder, and the bar, whereon the weights rested, was just placed in the middle of the piece of ice, between the two bearing cheeks, so that the line of pressure, the bar being round, was at six inches distance from each of the bearing cheeks; the broader part of the ice, was placed horizontal, and the narrower, was placed perpendicular. All things being thus fitted, we applied the weight to the two leavers of the engine, and began at fifty pounds; then mov'd them to 100, 150, 200, 250, and 300, suffering the weights to press the ice for some time, at every of these positions, the ice still bearing them, without breaking, or in the least crushing, either by the bearing cheeks, on which it rested, or under the round iron bar that rested on it; then removing the weights to 350, and suffering them to rest upon it, in a very short time, the ice broke short in two, just under the iron bar, though it did not appear at all to be crushed, at any of the three bearing places.

This experiment was tried, in order to find, first, the hardness of this body, which is produced by cold, out of the fluid body of water, without the mixture of any sensible solid body, or, is rather the primitive body, out of which, the fluid body of water is made, by a very small degree of heat, the difference between the greatest degree of heat, it will sustain without being thawed, and the least degree it will sustain without being frozen, being so very near the same, that one's sense will not discover it, and even a thermometer, but very little. So that if heat and cold, only, be the causes of these mutations, it is the greatest instance in nature of so considerable a change of texture, upon so inconsiderable an alteration of the causes.

Secondly, in order to find the tenacity or strength of this body for bearing, and thence, to give some reason, how it comes to bear so great weights, moved, or resting upon it, without being

broken, when it covers the top of a river or pond, as has been now sufficiently experimented upon the *Thames*. And though the manner of bearing, when the ice floats upon the water, be very differing from the way of bearing in this experiment, and so the calculation holds not the same in the one and the other; yet this way of trial is a necessary ingredient of such a calculation; since, without knowing the stiffness of ice, as to bending or breaking, and the hardness of ice, as to crushing, such a calculation cannot be perform'd. The case also varies very much from the manner of the boundings, and the bigness of the piece of ice, whose strength is to be calculated. For in a pond, where the edges of the ice are first frozen to the ground, and so the water underneath being pent in from being able to get out, the resistence of the water hinders the breaking of it, even till the resting weight begins to crush it. And 'tis much the same, where the surface of the ice is very large, though it no where toucheth or resteth upon a solid body at its brims, there being so great a length of water to be moved, before the water underneath can give way to the breaking of the ice. We must also consider the weight, as bearing in the center of a round flake, which is very differing from that of an oblong shape. To this calculation we must likewise take in the rising of those ambient parts of the ice, which at a distance encompass the bearing center, since the ice can hardly descend in the center, without at the same time raising some circumferential parts, which are more difficult to be broken upwards, than the center to be broken downwards.

An experiment of Dr. Hook's, *concerning the swelling of Water by freezing.* [Derham, p. 132.]

The third experiment I tried was upon occasion of a report of Dr. *Crone*, of an experiment try'd by himself, of applying the freezing mixture to a glass of water, and observing the water to rise in the neck of the glass, before any part of the water was frozen. Whence he conceived that the water itself did actually expand by its application, before it came to freezing. The reason of which phænomena I conceived to proceed only from the shrinking of the containing vessel, and not from the expanding of the water, before freezing: to elucidate which, I tried the

experiments I had formerly shewn, to prove the swelling of glass by heat, and the shrinking of it by cold; as also divers other phænomena, which are manifestly to be ascribed to the shrinking and swelling of the containing glass vessel, and not at all to the swelling and shrinking of the liquor contain'd; as the dipping such a glass of water, in hot water, will presently make the water descend in the neck; and the dipping the same in water colder, then the water in the glass, or then the glass it self, will make the same water rise for some time in the neck of the vessel. However, tho' some trials were made, whose effects seem'd, to me, plainly to concur with this explication, yet the doctor, and some others, seem'd yet to doubt, whether the water it self did not actually swell by the application of the freezing mixture, before it actually began to freeze; which if these trials do not satisfy, there may be several other ways made use of to find the swelling of the glass by heat, and the shrinking of it by cold. But I conceive no experiment can be made that will prove water, without freezing, to be dilated or expanded by cold, or contracted or condensed by heat.

Density of Ice.

Dr. CROONE said, that a piece of ice like a coffee-dish bore about $\frac{1}{4}$ of an ounce in water without sinking.

Mr. HOOKE said, that ice was $\frac{1}{8}$ lighter than water.

Feb. 13. Mr. HOOKE made a trial of the different weights of ice and water.[1]

Dr. HOOK'S *experiments, shewing the specifick Gravity of Ice, &c.* [Derham, p. 134.]

I took then a piece of metal big enough to sink the piece of ice, I designed to examine, to the bottom of the water, that so the compound body of ice and iron might have a sensible gravity in the water. Then letting it down into the water, which I had set conveniently in a glass, that I might see this compound freely to swim to and fro clear below the surface; the scales being conveniently sustained by a frame, I counterpoised it

[1] His account of these experiments is entered in the Register, vol. vi, p. 149, and printed in Mr. HOOKE's *Philosophical Experiments and Observations*, p. 134.

exactly to an equilibrium, and found it to amount to $1933\frac{4}{8}$ of 3000 parts of a pound troy, which were the weights to which I reduced this, and all the other counterpoises. Then I suddenly lifted up the ice and iron into the scale, and so counterpoised it in the air, and found the same to be $2567\frac{6}{8}$ of the same parts; then I took off the ice, dry'd the scale, and let the iron weight hang by the same string in the water; and counterpoising it, I found it to amount to $1984\frac{3}{8}$ of the same parts; then lifting the iron out of the water, and putting it into the scale, I found it to be counterpoised by $2209\frac{7}{8}$ of the same parts. Thence the weight of the water, equal in bulk to the ice and iron, was $634\frac{2}{8}$ of the like parts, and the weight of the water, equal to the ball, was $224\frac{6}{8}$; thence the weight of the water, equal to the ice was $409\frac{4}{8}$, and the weight of the ice in the air was $358\frac{5}{8}$, and consequently the weight of the ice in water was $50\frac{7}{8}$; that is, the weight of the ice, to that of the water, was very near, as 7 to 8; that is, the ice was lighter than the water, by an eighth part of the weight of the water; or the water heavier than the ice, by a seventh part of the weight of the ice. So that the expansion of the ice, to the expansion of the water, was as the weight of the water, to the weight of the ice; that is, as 8 to 7: so that the water, by its freezing, becomes expanded one seventh part of its bulk, and consequently that 7th part must float above the surface of the water, and $\frac{7}{8}$ of the bulk of ice must remain immersed in the water $\frac{1}{8}$ part of the bulk of the ice floating above it.

The ice I made use of, in this experiment, was not very full of blebs, or bubbles; nor was it perfectly free of them, but of a middling nature, which may pretty well hold, as a standard, or common measure of a great congeries of several sorts of ice, some of which may be much more porous, and some much less, as I have had occasion several times to observe, in this great frost. The time, in which I try'd this, was pretty warm, and so it thawed; and the water having stood all the day, exposed to the air, was consequently much of the same temper; and thence I counterpoised the ice and iron first in the water, and then presently lifted it out of the water into the scale, so that all that levitated in the water was immediately put in the scale: the water was ordinary pump, or well-water, and is accounted a

pretty good fresh water; which circumstances I mention, as having significancy, as will by and by appear.

For from this experiment it plainly appears, that the common opinion that the ice, upon a sudden thaw, sinks to the bottom, is false, tho' never so confidently asserted by the water-men: for in this experiment, where the water was pretty warm, in respect of ice, and thawed the ice very fast; yet an eighth part of the ice floated above the water, and water by heat, without boiling, will not expand near that proportion: nay, I have found, that throwing in a piece of ice into water boiling, it still floated, and sunk not, much less can it sink in a tepid water upon a thaw.

Next, from hence we may collect, that in the Northern Seas, at least one eighth part of the bulk of any body of ice floats above the water: I say, at least an eighth; for possibly it may be one seventh; for first (as is affirmed by many voyagers to the Northern Seas) the ice is found to be pretty fresh, and to have little or no taste of brackishness; and so, one part taken with another, not heavier than this ice I made use of. Next, the water, notwithstanding, in which it floats, is salt, and consequently about a 40th part heavier than common fresh water. Thirdly, this salt water, tho' it do not freeze, is yet pretty near the same degree of coldness with the ice that floats in it, and consequently yet more heavy than the same water when more tepid. For as I shall hereafter prove, bodies that freeze not, are yet not less cold than other bodies that do freeze. Fourthly, that the sea-water, near the bottom, is yet much more cold, and much more salt, than in the same place it is near the top, and consequently must much contribute to the floating of a greater part of the ice. That the water is colder at the bottom, than above, was positively affirmed by Mr. *Roachford*, who try'd it in the *Sound*; and that salt water is salter at the bottom, than at the top, any one may find.

All which particulars consider'd, it will not seem altogether so incredible, or indeed strange, that there should be floating islands of ice in the frigid zones, of so great a height above the surface of the sea: for, supposing it to be globular, above a 4th part of its diameter must float above the water, to make a 7th part of its bulk to float, and consequently the depth of the ice under water need not be so very great, to make so great a height

above the water; but if the upper parts of it above the water are yet much higher, and more spongy than solid ice, as consisting, in great part, of accumulations of snow, then may that height, above the water be raised much higher, and be made possibly to equalize, if not exceed, even the depth of the ice below the surface of the water, especially if the bottom of the said island be flat, as most probably it is, and as broad, if not broader, than the compass of it at the surface of the water; as also if parts above the water be tapering, like a pyramid, to the top. Again, if the lower parts of the sea, in those parts, are colder than at the top, as probably it may be in the spring, the fresher parts of the water may be congealed, even at the bottom, and so augment the bulk of it by new accretions underneath, and so continue to buoy it up more and more, and so raise the upper parts more and more in the air. And consonant to this we find, that the greatest islands of ice are found in the spring, after the winter is past, and the air begins to have a tepidness in it; and not so much, if at all, in the former part of the winter, when it freezes more violently at the top of the water.

As to the reason why water, when of such a degree of temperature, becomes so solid a body; and why, when of another temperature, it becomes so fluid, I shall not now spend your time in explaining, designing to do it in my General Theory of natural operations. This only I shall mention here, by the by, that the body of ice, tho' very hard, is very little sonorous, in respect of glass, which to the sight it so much resembles: that the blebs in it are not vacuities, but a kind of air, which has its expansive power, or elasticity, as well as common air: that this air does not, upon the thaw, retreat into the water, as it seems to come out of it upon the freezing, as by experiment I have found.

Deep Sea Water Fresh.

Feb. 20. Mr. HOOKE remarked, that this [that the bottom of the sea has been found fresh] was not general, but caused in some particular places by accident: that Captain KNOX being to go to the Indies, might make trial of it, if one of the engines, formerly invented by himself for taking up water at the bottom of the sea, were ordered to be made for the captain.

Mr. HOOKE said, that the sea-ice being something thawed before we had it, the salter part of the ice might be gone.

Experiments on Ice.

Mr. HOOKE showed some further experiments concerning the phenomena of ice.[1]

Feb. 20.

Farther experiments, made by Dr. HOOK, *before the Royal Society, concerning the Phœnomena of Ice.*

The proceeding discourse was read, and some matters therein more particularly explained by description partly, and partly also by experiments.

The experiments were first to shew, that the blebs in ice supposed by some to be vacuities, like the blebs in glass drops, are filled with air, which has the same properties with common air. I took then a piece of ice, and putting it into water, which was tepid, as having stood in a warm room, by which the outward parts of the ice quickly thawed, and so there remained nothing at all of air sticking to the outside of it; then whelming a cup-glass clear over it, which was perfectly filled with water, and had no air included in it, I suffered it to remain, covering the lump of ice, till the whole was thawed, or melted into water; and it was plain to be seen, that as the ice thawed, the blebs that were visible in it, before the thaw, did ascend to the top of the whelmed glass, and then unite with one another into a considerable body of air.

The second was to shew that water, though boiling hot, would yet be ponderous enough to make the ice to swim and float in it. This was done by putting a piece of ice into a vessel of boiling water: and the ice continued to float upon it till it was all melted.

The reason of the experiment was in order to find out the nature of the expansion of freezing water, and the true cause thereof; which seems to contain as many difficult phænomena in it to be explain'd, as any other in nature: for first, this body of ice seems heterogeneous to all other bodies; which being melted, and suffered to cool and grow hard, are still condensed and shrunk into a lesser and lesser room, as they grow colder; as is very observable in all sorts of metals, as gold, silver, copper, tin and

[1] There is no minute of this in the Journal Book, vol. vii, pp. 210–13, but Mr. HOOKE's account of these experiments is entered in the Register, vol. vi, p. 154, and printed in his *Philosophical Experiments and Observations*, p. 138.

lead, every of which, when they are melted, take up more space, or are more expanded, than when they are grown cold and hardened; as one may presently find, by casting any of them into a mould, and observing the setting, or shrinking of the gitt, by which the mould is fill'd; or by suffering the whole body, so melted, to remain, and grow cold and solid in the ladle or crucible; for 'tis evident that the top surface, which, when melted, is protuberant, and swelling upwards; when cold, it is flatted, and very often concave. And sometimes also, in some metals, it is crumpled, and shrunk into curious figures; as is very remarkable in *regulus martis*, made with *antimony*, which is therefore called *stellatry*, for that it hath some resemblance to the figure we generally make for a star, *viz.* six radiations from its center. 'Tis evident also in tin and lead; wax also, and some resinous substances, shrink upon hardening after the same manner, and fatt, or tallow of animals; so all sorts of vitrifications and glasses, and all sorts of oils, that will harden, and butter, which also grow opaque. But water, when it passes from fluidity to solidity, proceeds very differing; *first*, in its instantaneous change. *2dly*, in its expansion or rarefaction. *3dly*, in its transparency. *4thly*, in its refractiveness. *5thly*, in its generation of blebs, or bubbles. *6thly*, in its power of expansion: tearing and rending to pieces the strongest metalline bodies that imprison it; when, as yet, it leaves room enough for the small particles of air to expand, if at the same time it may not be said to suck it in; for I do not find that the imprison'd blebs are at all press'd, nor is their spring at all the cause of this expansion; for by observing the thawing of a bleb in the ice, I did not find the bubble that rose from it to be any bigger in bulk, than the bleb that contain'd it; whereas if the air in the bleb should be pressed with as great a force, as the strength of the inside of the containing vessel amounts unto, it must of necessity reduce the air to near a thousandth part of its natural extension; and consequently, when the bleb comes to be thawed, and so set at liberty, it must at least, I say at least (by reason it then suffers a greater degree of heat, than when it is frozen) expand itself into a bulk a thousand times bigger; but there is no such appearance that I could observe. Several authors have endeavour'd to give solutions of this phænomenon, as particularly

the ingenious Mr. *Des Cartes*, who supposing the particles of water to be very long and limber bodies, like so many eeles, whilst, as it were, kept alive, and agitated by this *materia subtilis*, are limber, and so easily complicate and slide one within another, and suffer the *materia subtilis* to have its passage free through them every way; but when there is less agitation of this *materia subtilis*, they do, as it were, die, and grow stiff and rigid, and so will not so easily comply to the figures of each other, but grow solid and hard: but then 'tis to be consider'd, that the greater plenty there is of the *materia subtilis*, the greater must be the agitation of them; as he asserts in the explication of the particles of the air, and consequently the more room must they take up, and so be more expanded when fluid, then when solid. Another late author supposes, that congelation is made by a *sal armoniack*, breathed, or exhaled from animals, which, in cold, frosty weather, is very copious in the air, which *sal armoniack* does then insinuate into the pores of the water, and so wedge up all the pores, and widen them, and so make the parts of the water to coalesce into a hard body. But this I conceive to be also hypothetical, and not experimentally proved; for tho' there may be some volatile salts in the air, yet 'tis pretty difficult to conceive there should be so great a quantity, as at once to wedge up all the water of the northern part of the earth, and yet, at the same time, we should not smell it; besides, we do not find that the *sal armoniack* spirit does perform this effect, when it is raised in the air at other times; nor does the *sal armoniack* it self, when mixed with water or ice, do it; for we find that *sal armoniack*, strow'd on ice, will the sooner make it thaw, and resolve again into water, than make it freeze harder: others have given differing explanations, but I have not met with any yet, that, in my opinion, give a clear and satisfactory solution of it. Nor shall I at present trouble you with theories, or speculations, which some may possibly have a prejudice against; only suffer me to acquaint you with a phænomenon or two, which, if you think any of them worth seeing, you may have tried, for they are very obvious, plain, and neither difficult nor chargeable experiments, tho' possibly as instructive as the most difficult, chargeable, or pompous experiments, to shew some sorts of expansion.

Take then a urinal, and fit into it a stopple of a dry piece of wood; then put the end of this stopple into a dish of water, and you will find, in a little time, the stopple will grow so much bigger, as to break the urinal.

Secondly, take another urinal, and fill the same with pease; then filling it up with water, stop the same with a cork, which you may tie down fast with a packthread; then let it remain some time, and you will find the pease will swell and break the glass.

Thirdly, take plaister of *Paris*, or burnt alabaster, and put into a wooden dish, and temper it with water, till it be very soft and fluid, that it may be easily poured out; then with this mixture fill a urinal or vial top-full, suffer it to stand upright till it sets into a solid body, and you will find it swell and break the glass.

Hooke's Minutes.

Feb. 27. The books in the Secretary's keeping were viewed, and found to include:

Three stitched paper-books of minutes taken by Mr. Hooke, beginning October 25, 1677, and ending February 23, 1680/1. Another bound book of Mr. Hooke's minutes, about one-fourth full, beginning March 2, 1680/1, and ending July 26, 1682.

It was resolved, that the minutes of Mr. Hooke be written in books suiting with the rest.

Fall of Barometer before Wind.

Mr. Hooke remarked, that before the wind, which began the 7th at night, the quicksilver fell lower than it had been in seven years before: and that the wheel-barometer almost made a full turn in less than three days.

Dr. Croone's 2nd Experiment on Expansion of Water in Glass.

Upon mention of this experiment in the society, Mr. Hooke objected, that it was the contraction of the glass, that raised the water, and that likewise a quantity of the water in pouring in stuck to the sides of the stem, which by little and little falling helped to make the water rise in it; therefore first another bolt-head was taken, and put empty into a frigorific mixture of salt and ice (for now the frost and snow were just gone) that it might first shrink and contract there as much as it could; likewise another bolt-head at the same time was filled half-way with water, and set in another vessel of the same mixture, and when it had

rose about two inches, and was thought near freezing, this infrigidated water was instantly poured into the first bolt-head to prevent another objection, that it might of itself have been so warm as to have relaxed again the empty glass when it was poured in; and also to cut off the other part of the former objection, that some part of the water poured in sliding after down the sides of the stem, might help to raise the water there, a long glass funnel was provided, that reached down to a mark set on the stem of the empty bolt-head, into which the water was to be poured: after all, the effect was, that the water began instantly to rise as before, and when it was got up about ¼ of an inch, we took it out, to see if there were no ice, fearing it might have begun to freeze in the ball, but it was still all fluid.

Change of State of Water.

Mr. HOOKE showed the way, which he took to examine the limits of heat and cold, that water will endure in the guise of a liquor, beyond which degrees, if the heat were increased, it turned to the spirituous body of air, and both by a kind of instantaneous starting or fulmination; the one to a prodigious expansion of some hundred of times its watery guise; but not so powerful suffering condensation as well as air; the other only starting about an eighth or seventh part, but so powerful, as to induce no compressing from almost the strongest body. He examined then by weighing an iron ball in it, both when it was just freezing, and when it was just boiling; and by that he certainly found, that in the state of water it was capable but of one-thirtieth part of its bulk to be extended; that is, that the same quantity of water boiling hot, and so ready to turn into the form of air, was but a one-and-thirtieth part lighter than the same bulk of water, when it was ready to turn to ice.

Magnetism of Heated Steel.

He then tried several magnetical experiments about the quenching of red-hot rods of steel.[1]

Drilling and Magnetism.

Mar. 5. Some magnetical experiments[2] having been appointed to be tried by Mr. HOOKE before the society, the effect was as follows:

There were three drills made of steel, and well hardened, and then fitted with pulleys for drilling.

[1] There is no entry of these two sets of experiments of Mr. HOOKE in the Journal of February 27, 1683/4, but his account of them is registered in the Register, vol. vi, p. 158.

[2] Dr. LISTER had read a paper on Magnetism at the previous meeting. HOOKE's MS. is preserved in R. S. MS. 60.

Each of these, before they were used, were examined by a small magnetical needle in a box, to see, whether by their hardening they had acquired a fixed magnetical virtue, but by often repeated trials they were found not to have any such virtue; but which end soever was downward attracted the south end of the needle, and the upper end always attracted the north.

Each of these drills were severally tried, one of them by drilling of brass; a second by drilling of copper; and a third by drilling of marble; in every one of which in the drilling the edged point of the drill was turned dipping towards the north.

These drills being afterwards examined by the same needle in the box, they were found not to have any sensible polarity more than they had before the drilling; but which end soever of any one of them was turned downward, that was found to attract the south end of the needle in the box, and to chase away the north; and which end soever was uppermost, had the contrary effect.

Mar. 12. Mr. HOOKE examined several drills, some of which had been long used in ships. The bit was a strong north pole; the shank of some drew indifferently both poles of a needle very weakly.

Congers.

Mar. 19. Mr. HOOKE remarked, that congers were taken in pots off the Isle of Wight, and the Scilly Islands.

Magnetism of Heated Iron.

Mr. HOOKE showed how a piece of iron heated red-hot drew the south pole of a needle more strongly than if it were cool, and repelled the north pole: And that the end of a piece of iron quenched downwards was a strong north pole; and quenched upwards a south pole.

Mr. HOOKE declared, how he had taken half an iron ring, and quenched it perpendicularly with the ends downwards; and that the ends were two weak north poles, and the middle a weak south pole.

Hooke's Magnetical Experiments.

The account drawn up by Mr. HOOKE of his magnetical experiments tried with several rods of steel without touching them on the loadstone, as exhibited at this and three preceding meetings of the society, was as follows:

I showed the way I took to examine the limits of heat and cold, that water should endure in the guise of a liquor, beyond which degrees if the cold was increased, it turned to the solid

body of ice; if the heat was increased, it turned to the spirituous body of air, and both by a kind of instantaneous starting or fulmination, the one to a prodigious expansion of some hundred of times its watery guise, but not so powerful, suffering condensation as well as air, the other only starting about an 8th or 7th part, but so powerful as to endure no compression from almost the strongest body. I examined then by weighing an iron ball in it, both when it was just freezing, and when it was just boiling; and by that I certainly found, that in the state of water it was capable but of one-thirtieth part of its bulk to be extended; that is, that the same quantity of water boiling hot, and so ready to turn into the form of air, was but a one-and-thirtieth part lighter than the same bulk of water when it was ready to turn to ice. After which I tried several magnetical experiments about the quenching of red-hot rods of steel: as I did also March the 5th, 12th and 19th, the account of which follows:

Magnetical experiments tried with several rods of steel without touching them on the loadstone.

They were of three sorts, 1st, on rods heated; 2nd, on rods quenched; 3rd, on rods hammered.

First, I found, that small rods of steel about $\frac{1}{8}$ of an inch square, and six or eight inches long being heated red-hot in a fire were much more receptive and communicative of the magnetical virtue of the earth than the same when cold; that is being held perpendicular, or rather in the true dipping posture, the magnetical virtue did more powerfully actuate a needle, the lower end attracting the south, and the upper end the north part of a magnetical needle applied near to either of them; and that the same rod red-hot being inverted performed the same effect. And that the virtue continued of the same nature, though more weak, when they were grown cold.

2. That a polarity acquired by hammering or drilling would be destroyed by a heating of the rod all over red-hot; and when suffered to become cold, they retained the same indifferency without their former determined polarity.

3. That heat abstracted from steel was not the cause of this effect, for that a red-hot tobacco pipe heated and applied in the same manner had not such effect, nor burning wood coals, nor the flame of a candle applied very near.

4. That I heated one of those rods red-hot, after it had been touched by the magnet, and so had a strong polarity, which would be whilst red-hot reduced to its natural indifferency, and the lower end would attract the south end of the needle, and the upper, the north; and this indifferency it did retain when cold, being suffered to cool leisurely.

5. That I found the magnet did as powerfully attract and hold the rod when red-hot as when cold.

Secondly, I found, that these rods of steel being heated red-hot, and then quenched in a perpendicular posture, or in the posture proper for the dipping-needle, the lower end, which was the quenched end, would acquire a polarity, and attract the south end of the needle; but the other end of the rod seemed almost indifferent to either pole, when inverted and turned downwards; but the quenched end, though it were turned upwards, would attract the south end of the needle.

Next I took a vessel full of water, the bottom of which was nothing but a thin piece of parchment: then I heated a rod of steel, and when glowing hot I ran the upper end of it placed in the dipping posture through the parchment bottom into the water, and thereby hardened the upper end of the rod; then I applied a needle to it, and found, that the quenched end had acquired a polarity, and attracted the north end of the needle, whether it were held upwards or downwards, but the other end seemed to have acquired not near so much, but seemed almost indifferent, in attracting the south when held downwards, and the north when held upwards.

3. I heated one of these rods of steel glowing hot, and through the parchment now made the side of the vessel, I run it into the water directly pointing towards the east in a horizontal posture, to see whether the quenching of the end abstractedly considered, as sudden cooling, would give any polarity to it; but I found, that the quenched end had now no more virtue or polarity than the other end, but the rod remained indifferent in both its ends; that which was downwards attracted the south, that which was upwards attracted the north.

4. I heated the rod again glowing hot, and quenching it flat-ways in the water as near as I could in the east and west posture; and examining the same by a needle, I found it had not acquired any fixed polarity at either end, but each of them remained indifferent.

[5]. I heated a rod of steel as before, glowing hot, and quenched it with an inclination in the plane of the meridian at right angles with the dipping line, and examining it by the needle I found, that neither the quenched end nor the other had acquired any fixed polarity, but remained indifferent as before.

Thirdly, I tried several rods of steel, which had been thoroughly heated, and then suffered it to cool leisurely, lying east and west, and having found them indifferent without polarity, I put one of them in the posture of the dipping-needle pretty near the plane of the meridian: I hammered it at the upper end with a hammer, the lower end resting in a hole in an anvil: then

examining it with a needle, I found, that both its ends had acquired a pretty sensible polarity; the under end a north, and the upper end a south; but the upper end seemed somewhat the stronger.

2. I inverted the rod by turning the north end upwards and the south end downwards, and hammering it in this dipping posture a good while, I found, that it had acquired a quite contrary polarity to what it had before; that end, which was before the north end, being now the south, and the south end was now the north.

3. I took another rod of steel, which was indifferent, and having placed an anvil so, that the upper face of it respected the north with an inclination pretty near the posture of the dipping-needle, I laid the rod upon the same pretty near in the plane of the meridian, and then hammered the middle of the said rod with a hammer for a good while, and examining the rod afterwards with a needle, I found the rod had acquired a polarity, the lower end a north, and the upper end a south.

4. I hammered another rod of steel at right angles to the former position, the ends thereof respecting the east and west; and I found after this hammering, that neither end had acquired any fixed polarity, but they both seemed to remain indifferent as before.

1684

Snow Crystals.

Mar. 26. Mr. Hooke said, that snow had a growth like a plant; that he had observed it in stems with branches and leaves like that of a trefoil.... It was not a vegetation, but an accretion.

Durability of Oak.

Mr. Hooke said, that oaken boards might continue a hundred years.

Mushrooms.

Mr. Hooke observed, that there had been some hundreds of species [of mushrooms] mentioned, and that there was no doubt of their having seed.

Glass-making and Manganese.

The making of glass being further discoursed of, Mr. Hooke said, that manganese is iron ore.

Mr. Hooke remarked, that the Roman glass hath [their] colour by age, because the glass in old abbeys seemed to be of the same colour.

Mr. Hooke said, that jet for glass painting might be made by

lead alone, but that it is very troublesome, running through all the pots.

The Weather-clock.

Apr. 2. It was ordered, that Mr. HOOKE be desired to put into writing a description of the weather-clock and all its parts; and that it be delivered to the Secretary to be entered in the Register Book:

That Mr. HOOKE give his directions and assistance to Mr. HUNT, to reduce into writing some of the first papers marked by the weather-clock, that thereby the society might have a specimen of the weather-clock's performances before they proceed to the repairing it.

Hooke's Remarks on Flamstead.

Mr. FLAMSTEAD desired, that the Journal Book in 1682 might be altered as to some expressions reflecting upon him entered by Mr. HOOKE.

Ice Crystals.

Mr. HOOKE mentioned the shooting of ice near a mixen, and urine on the side of a glass (filled with snow and salt) in hexangular figures.

Mushroom Seeds.

Upon mentioning the seeds of mushrooms, it was said, that mushrooms upon trees might be the matrices of insects, and not come from a seed of their own.

Mr. HOOKE thought that they had seed; and it was instanced in the Filix kind, which was formerly thought to be without seed.

Fairy Rings.

Mr. HOOKE observed, that he had seen fairy-circles on chalky hills, where he thought there was not ground enough for the moles to hide themselves.

Ancient Glass.

Upon mentioning the difference of glass urns from modern glass, Mr. HOOKE conceived, that they were altered by lying.

The urn given by Sir CHRISTOPHER WREN being called for and viewed, was found very smooth, but in some places had a bluish shining.

Mr. HOOKE observed, that this was a sign of its readiness to scale: that he had formerly seen several scales, that came out of it: and that Sir CHRISTOPHER WREN thought, that the mark of the puntillion might be fallen off with the scurf.

Growth of Timber.

Mr. HOOKE observed, that a new circle of wood being added to a tree every year, the trees might be closed on the outside, however they were loose and open within.

Mr. HOOKE thought, that the trunk being the older part might die before the top.

Decay of Timber.

Mr. HOOKE thought, that the proper menstruum, which decays [wood], was taken away [in charcoal], and that therefore [charcoal] lasted.

Hardening of Wood.

Apr. 9. Mr. HOOKE remarked, that turpentine in knots was one cause of making them hard; and that white deal, soaked in turpentine, becomes very hard.

Weather-clock.

Mr. HOOKE brought in a short description of the weather-clock, to which he was desired to add the figures of such parts, as could not be well understood by the words, and to put down the key for expounding the papers marked by the engine.

[Cf. figure on p. 162.]

£7 10s. for Experiments.

Apr. 16. It was ordered, that Mr. HOOKE be paid seven pounds ten shillings for the experiments brought in by him as curator till Christmas last.

That he bring in a written account of the experiments made by him since Christmas; and that then the council would order his payment accordingly.

Tides at Tonquing.

Apr. 23. Mr. BAILEY having delivered in an account of the tides at Tonquing, procured from persons, who had lived long in the place; it was ordered to be registered.

It was very remarkable, that there was at Tonquing but one flood and ebb in twenty-four hours; and, as Mr. HOOKE observed, that when the moon is in the north of the equator, the floods begin in the morning: When she is in the south side of the equator, they begin in the afternoon.

Mr. HOOKE remarked likewise, that Captain KNOX had made several observations confirming the truth of this account, as would appear from his journal in the hands of the Earl of CLARENDON, if it were consulted.

Parisian Mathematical Engine.

Apr. 30. A letter of Mr. MUSGRAVE to Mr. ASTON, dated at

New College, Oxford, April 22, 1684, was read, concerning a mathematical engine lately invented at Paris, made very commodious for travelling, and so light, that it might be carried in the pocket, serving for a semicircle, sector, or square, measuring all sorts of angles whatever, taking the weight of bullets, the declination from the north, the inclination or reclination of any wall, &c.

Upon reading of Mr. MUSGRAVE's letter, Mr. HOOKE remarked, that a sector with sights might perform whatever the new mathematical engine at Paris was said to do.

A Hermetically Sealed Barometer from Rouen.

May 14. A barometer being brought, Mr. PIGOT held the flame of the candle to the upper part of the quicksilver, and after a little time the quicksilver sunk $\frac{1}{4}$ of an inch or $\frac{2}{10}$ $\frac{1}{2}$.

Mr. HOOKE thought, that the glass might not be well cleansed of the air, and proffered to get one better cleansed against the next meeting.

Signalling.

May 21. Mr. HOOKE read a discourse showing the way how to communicate one's mind at great distances.[1]

Dr. HOOK's ***discourse to the Royal Society, shewing a way how to communicate one's mind at great distances.*** [Derham, p. 142.]

That which I now propound, is what I have some years since discoursed of; but being then laid by, the great Siege of *Vienna*, the last year, by the *Turks*, did again revive in my memory; and that was a method of discoursing at a distance, not by sound, but by sight. I say therefore 'tis possible to convey intelligence from any one high and eminent place, to any other that lies in sight of it, tho' 30 or 40 miles distant, in as short a time almost, as a man can write what he would have sent, and as suddenly to receive an answer, as he that receives it hath a mind to return it, or can write it down in paper. Nay, by the help of three, four, or more, of such eminent places, visible to each other, lying next it in a streight line, 'tis possible to convey intelligence, almost in a moment, to twice, thrice, or more times that distance, with as great a certainty, as by writing.

For the performance of this, we must be beholden to a late

[1] It is printed in his *Philosophical Experiments and Observations*, p. 142.

invention, which we do not find any of the antients knew; that is, the eye must be assisted with telescopes, of lengths appropriated to the respective distances, that whatever characters are exposed at one station, may be made plain and distinguishable at the other that respect it.

First, for the stations; if they be far distant, it will be necessary that they should be high, and lie exposed to the sky, that there be no higher hill, or part of the earth beyond them, that may hinder the distinctness of the characters which are to appear dark, the sky beyond them appearing white: by which means also, the thick and vaporous air, near the ground, will be passed over and avoided; for it many times happens, that the tops of hills are very clear and conspicuous to each other, when as the whole interjacent vale, or country, lies drowned in a fog. Next, because a much greater distance and space of ground becomes visible, insomuch that I have been informed by such, who have been at the top of some very high mountains, as particularly at the top of the *Pike of Teneriff*, that the island of the *Grand Canaries*, which lies above 60 miles distant, appears so clear, as if it were hard by; and I myself have often taken notice of the great difference there is between the appearing distance of objects seen from the tops and bottoms of pretty high hills, the same objects from the top appearing nearer and clearer by half, and more than they do when viewed from lower stations of the hills; and this not only when the space between them was land, but where it was nothing but sea. I have taken notice also of the same difference from the prospect of places from the top of the column at *Fish-street-Hill*, where the eye is, in good part, raised above the smoaky air below.

Next, the height of the stations is advantageous, upon the account of the refractions or inflections of the air; which inflections of the air are many and very great, sometimes in an air which seems, to the naked eye, the most clear and serene. Insomuch that that alone does wholly confound the distinctness of objects appearing at a distance; now the greatest part of these arise from commotions of the more dense air that is near the surface of the earth, by the rarefactions of some parts of it, caused by heat; which rarefied parts ascending, do make the

objects seen through it, to seem to dance and undulate, which is in great part avoided, if the prospect be from an higher place. Besides, the nature of the air itself, at great heights, approaches nearer to the nature of the *æther*, which more powerfully propagates the impulses of light.

Next, in chusing of these stations, care must be taken, as near as may be, that there be no hill that interposes between them, that is almost high enough to touch the visible ray; because in such cases, the refraction of the air of that hill will be very apt to disturb the clear appearance of the object, as I have often observ'd.

The stations being found convenient, the next thing to be consider'd, is, what telescopes will be necessary for such stations. And though 'tis true in all, that the longer the telescopes are, provided they are good, the better they will be for this effect; yet somewhat of limitation is requisite, at least, that they be not shorter than certain limits for several distances. These may be as follows: for 1 mile, 1 foot; for 2 miles, 2 foot; for 3 miles, 3¼ foot; for 4 miles, 4½ foot; for 5 miles, 5 foot 10 inch.; for 6, 7¼ foot; for 7 miles, 8 foot 9 inch.; for 8, 10½ foot; for 10 miles, 13 foot, and so forward. One of these telescopes must be fix'd at each extreme station, and two of them in each intermediate; so that a man, for each glass, sitting and looking through them, may plainly discover what is done in the next adjoining station; and, with his pen, write down on paper the character there exposed, in their due order; so that there ought to be two persons at each extreme station, and three at each intermediate; so that, at the same time, intelligence may be convey'd forwards and backwards.

Next, there must be certain times agreed on, when the correspondents are to expect; or else there must be set at the top of the pole, in the morning, the hour appointed by either of the correspondents, for acting that day; if the hour be appointed, pendulum clocks may adjust the moment of expectation and observing. And the same may serve for all other intermediate correspondents.

Next, there must be a convenient *apparatus* of characters, whereby to communicate any thing with great ease, distinctness and secrecy. There must be therefore, at least, as many distinct characters, as there are necessary letters in the alphabet that is

made use of, (as is expressed in *Fig.* 1). And those must be either day characters, or night characters: if they are to be made use of in the day-time, they may all be made of three slit deals, moving in the manner I here shew, and of bigness convenient for the several distances of the stations for which they are made, that they may be visible through the telescope of the next station. Any one of which characters may signify any one letter of the alphabet, and the whole alphabet may be varied 10000 ways; so that none but the two extreme correspondents shall be able to discover the information convey'd; which I shall not now insist on, because it doth more properly belong to Cruptography. If the characters are for the night, then they may be made with links, or other lights, disposed in a certain order, which may be veiled, or discovered, according to the method of the character agreed on; by which, all sorts of letters may be discovered clearly, and without ambiguity.

There may be various contrivances to facilitate and expedite the way of displaying and exposing these characters to view, and of withdrawing, or hiding them from the sight; but this I here shew, I conceive, will be as easy and simple as any: all which may be exposed at the top of a high pole, and by two small lines moved at the bottom, so as to represent any character.

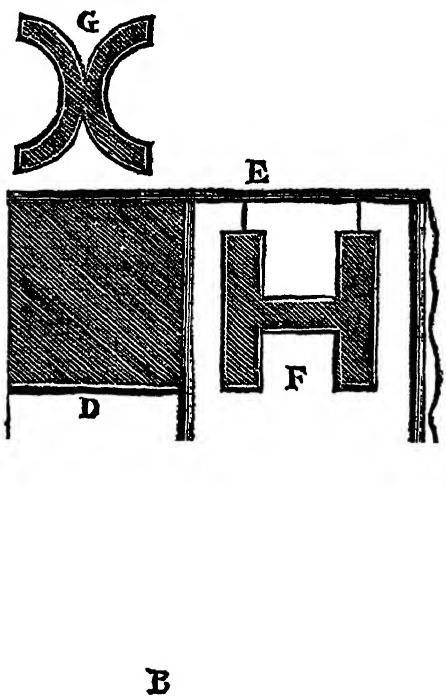
By these contrivances, the characters may be shifted almost as fast, as the same may be written; so that a great quantity of intelligence may be, in a very short time, communicated.

There will be also requisite several other characters, which may, for expedition, express a whole sentence, to be continually made use of, whilst the correspondents are attentive and communicating. The sentences, to be express'd by one character, may be such as these, in *Fig.* 2.

○ *I am ready to communicate.*)(*I am ready to observe.* (*I shall be ready presently.*) *I see plainly what you shew.* ◡ *Shew the last again.* ⌒ *Not too fast. Shew faster. Answer me presently.* Dixi. *Make haste to communicate this to the next correspondent. I stay for an answer*; and the like.

All which may be express'd by several single characters, to be expos'd on the top of the poles, by themselves, in the following manner, so as no confusion may be created thereby,

I could instance in a hundred ways of facilitating the method of performing this design with the more dexterity and quickness, and with little charge; but that, I think, will be needless at present, since whensoever such a way of correspondence shall be



put into practice, those, and many more than I can think of at present, will of themselves occur; so that I do not in the least doubt, but that with a little practice thereof, all things may be made so convenient, that the same character may be seen at *Paris*, within a minute after it hath been exposed at *London*, and the like in proportion for greater distances; and that the characters may be exposed so quick after one another, that

a composer shall not much exceed the exposer in swiftness. And so great expedition may not only be performed at the distance of one station, but of a hundred; for supposing all things ready, at all those several stations, for observing and exposing, as fast as the second observer doth read the characters of the first exposer; the second exposer will display them to the observer of the 3rd station, whose exposer will likewise display them for the 4th observer, as fast as his observer doth name them to him, or write them down.

There may be many objections brought against this way of communication; and so many the more, because the thing has not yet been put in practice. But, I think, there can hardly be any so great, as may not easily be answered and obviated.

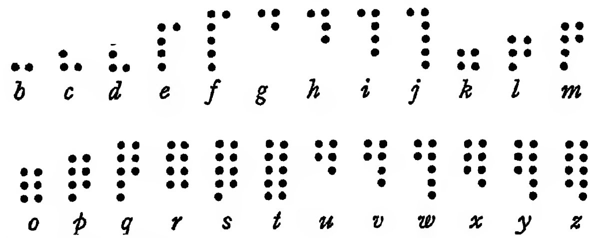
There may be many uses made of this contrivance, wherein it will exceed any thing of this kind yet practised; but I shall not now spend time to enumerate them; only in two cases, it may be of inestimable use. The first is for cities or towns besieged; and the second for ships upon the sea; in both which cases, it may be practised with great certainty, security, and expedition.

A farther Explication of the Figures.

Let A B C represent three very long masts or poles erected. E the top-piece, that joins them all together. D, a screen, behind which, all the deal-board characters hang upon certain rods or lines, and may (by the help of small lines coming down from the bottom of each of them) be exposed at F, or drawn back again behind D, as occasion shall be. G is the character for a sentence agreed on, *&c.*

The Letters of the Alphabet in Characters.

All the alphabet, or requisite characters, may be distinctly, and without ambiguity, expressed. Such a disposition as this, which I have here describ'd, I think, will be sufficient.



Torricellian Experiment.

May 28. Mr. HOOKE read a paper concerning the reason of the quicksilver's standing sometimes far above the usual height in the Torricellian experiment, in this case at 75 inches.
[*Posthumous Works*, pp. 365–70.]

Hydrostatical Experiment.

He also showed, that a glass pipe closed at the top with a great many very small pipes, when it was put in water up to the top, and there raised, kept the water suspended a good way above the level.

n. d. Of a Telescope stand set up in Gresham College. (9 pp.) [R. S. MS. No. 61.]

June.

Astronomy's Advancement, or News for the Curious . . . the best way of using Instruments for satisfaction etc.; out of the best Astronomers viz Mr. Hooke, etc.

Long Telescope.

June 25. Mr. HOOKE read a paper of remarks about the manner of observing with long telescope-glasses without a tube.
[Cf. R. S. MS. No. 62.]

Ancient Rowing.

July 2. Mr. HOOKE read a discourse concerning the manner of rowing used by the ancients in their galleys; which he was desired to deliver in writing with a draft.[1]

Porsenna's Tomb.

July 9. Mr. HOOKE read a discourse concerning the form of

[1] Printed *Posthumous Works*, pp. 568–72.

PORSENNA's tomb, described in PLINY. He also showed a scheme of it different from that of Mr. GREAVES, of which he was desired to leave a copy.

Hour-glasses Uncertain.

July 16. There was read a translation of a Dutch paper about finding the longitude by a twenty-four-hour glass.

Mr. HOOKE remarked, that that sort of time-measurers had been found very uncertain by several trials; and therefore laid aside.

Ancient Rowing.

Mr. HOOKE read a further explanation of his opinion about the galleys of the ancients, which he confirmed by a figure engraved on TRAJAN's pillar representing the manner of rowing.

Flamstead's Complaint.

July 23. Upon a complaint of Mr. FLAMSTEAD, that he had been reflected upon by Mr. HOOKE in the minutes of the society, it was ordered, that a line should be drawn through the places complained of, and that there should be written on the side, *cancelled by order of council*: and that the Journal Book should be brought to the next meeting of the council, who should see it done.

Nov. 6. *Art of Navigation.* [*Posthumous Works*, pp. 475–81.]

Nov. 13. *Terrestrial Magnetism.* [loc. cit., pp. 481–3.]

Nov. 20. Lecture on *Use of the Globes* and ways of finding the *Meridian* and *Latitude.* [loc. cit., pp. 486–94.]

Hooke Elected on to the Council.

Dec. 1. Then were chosen out of the society ten persons to complete the new council, including Mr. HOOKE.

New Level.

Dec. 10. Mr. HOOKE showed the draft of a new level invented by himself, which, he said, exceeded all those lately mentioned in the book published by Monsieur DE LA HIRE.

It was ordered, that Mr. HUNT take directions from Mr. HOOKE for the making that level against the next meeting, that it might be then tried.

Dr. Vossius's Book. The Moon has no Atmosphere.

Dec. 17. Mr. HOOKE read a paper of observations concerning Dr. VOSSIUS's late book; and upon this occasion mentioned some reasons, why the moon had no atmosphere.

[R. S. MS., No. 63.]

1684/5

Vomiting Worms.

Jan. 7. Mr. HOOKE remarked, that Mr. WHITE the chemist at Oxford had been troubled with the falling sickness, and by a vomit had brought up several worms; but he could not tell the sort, referring himself to the person living at Oxford.

Watered Silk.

Jan. 28. Mr. HOOKE showed a piece of serge, which one Mr. HASKINS in Jewen Street had caused to be watered as silks used to be.

Smethwick's Glass-grinding Engine.

He showed likewise a draft of Mr. SMETHWICK's engine for grinding glasses; for which Mr. SMETHWICK had a patent granted him. This draft was delivered to Mr. HUNT in order to take a copy of it.

Carriages.

Feb. 18. Mr. HOOKE read a paper concerning the different ways of carriage, which there are upon land and water.

Art of Water Carriage. [R. S. MS., No. 64.]

It may have been in connexion with this subject that about this time he put together a few notes on *The Movements of Turtles in Water* (3 pp.). [R. S. MS., No. 66.]

Feb. 25.

Dr. HOOK's *discourse of carriages before the Royal Society, with a description of* STEVIN's *Sailing Chariot made for the Prince of* ORANGE. [Derham, p. 150.]

The occasion of this following discourse was from the module of a *waggon*, shew'd to the *Royal Society*; upon which Dr. *Hook* discoursed largely on the various ways of conveyance. Among all which he saith, but that which excelled any, that has hitherto been done of that kind, was the sailing chariot, made by *Simon Stevin*, for the Prince of *Orange*, which, in two hours' time, ran upon the sand, on the sea-shoar, by the strength of the wind, forty two miles, carrying in it no less than 28 men, with safety and security: of which I have seen the description, and have had the full account. But this being only accommodated for such smooth ways, as the sand on the sea-shoar, could not be made for common use, and has therefore been laid aside and disus'd.

However, since there is a possibility of such a performance, it may, perhaps, be worthy consideration and further enquiry, whether it may not be possible to contrive, and make some other kind of chariot, or carriage, which may perform as much in any other passable ways; which, I conceive, would be of vast benefit to mankind.

The principal matter, wherein it differ'd from all other sorts of land-carriage, was this, that instead of making use of the *strength* of *men*, or of any sort of *animal*, he made use only of the *strength* of the *wind*, and that after the same manner as it was then made use of, for the moving of vessels upon the water; namely, by having masts, sails, and other convenient rigging, as shrouds, stays, sheets, booms, and all other rigging, as was necessary for the management of those sails. Then, for guiding this engine, he so order'd his contrivance, that he could, by turning the axle-tree of the wheels, make it go this way, or that way, at pleasure, with as much ease and certainty, nay, very much more than 'tis possible to steer a ship, or any other vessel upon the water. To keep it safe and secure from overturning, though on so smooth a plain as that passed over, there was little danger from the rising of the wheels on one side; yet, in the first attempts, it being better to overdo, in making provisions against any thing of danger, he placed the wheels at a great distance, or breadth, one from another; and, as I judge by the draught, very near half the length of the whole carriage; by which means there could be no manner of danger in over-setting; and still the less, the more the carriage was loaden, if the danger of over-setting were to be feared from the strength of a side wind upon the sails; for the wheels being placed at a pretty distance without the body of the carriage, all the weight of the carriage, together with the weight of two of the wheels, and all the weight of the men must be lifted up, and rest upon the two wheels on the leeward side, which neither sails nor masts would be able to do.

The way of steering or guiding this carriage, was much the same with that which is, and has always been practised in carriages upon 4 wheels; namely, an helm, or pole, so fastened to the *axis*, that by the means thereof, as by a leaver, the axis could be swashed either this way, or that way, upon a center-pin,

as is now in use in coaches and waggons, for the turning or swashing of the fore-axis; only, whereas the pole, in these, is turned and extended outwards, before the carriage, in this, it was turned inwards. The wheels are about a middle size, between the usual size of the fore and hind wheels of a coach, and were made very strong and substantial; and what was peculiar in them, was, that the rims of them were 18 inches, or 2 foot broad, and the spokes were made to strengthen the whole breadth; the reason of which I suppose was, that they might thereby be the better able to rest upon the sandy shoar, without sinking, or making rotes in it, which would have made it move very much heavier, the wheels being thereby always in a rising motion; for the weight of the whole carriage, and the weight within it (which must be very considerable, there being 28 persons in it) resting only upon the four points of the wheels; if they had been made with narrow rims, must necessarily have sunk pretty deep into the sand; but being broad, and the sand very smooth, as it is generally left by the sea, a small sinking of the touching line of the breadth of the wheel, doth presently make a very broad footing, to rest upon the sand.

There were two of these chariots made, the one a larger, of about 30 foot long, and the other a smaller, about 10 or 12 foot long: the larger had two masts and two sails, proportionable to the sails of a boat, much about the same bigness. The lesser had only one mast and one sail, proportioned likewise to its bigness. Each of the sails had two yards, the one at the top, and the other at the bottom, with proper rigging to work them. The bottom yard, I conceive, was put upon a double account, first, to keep the sail more flat and plain, that it might, when the carriage was to sail near a wind, be kept more sharp and trim; the great advantages of which I endeavour to prove upon another occasion.[1] And secondly, that the sails might be the easier managed, and tacked, as occasions should require. And though I cannot find, whether this engine was ever tried, or made use of, for sailing by a wind; yet, I doubt not, but that it would have far exceeded any vessel whatsoever, that sails upon the sea, in going near a wind; because, that in this, there could be no falling to *leeward*,

[1] *Posthumous Works*, p. 564.

(which the best vessels on the sea do more or less) the wheels, in this, keeping it directly in the line, or plain of the wheels.

The greater carriage was guided, or steered, by moving the hinder wheels by a pole, like the helm in a ship, and the end of it had tackles to bend it towards this or that side; and the rule of steering was the same as in a ship. The lesser carriage was steered by moving or turning the axis of the fore wheels; the pole or helm being turned backward into the carriage, and the rule of moving it was also the same as the former.

The last thing to be considered in these carriages, is the great swiftness of their course, which was so considerable, that no horses, in their full speed, could long keep pace with them; and vessels on the sea, sailing the same way, seem to be carried backwards very swiftly. This, had it not been attested by testimonies of undoubted credit, would have seemed very difficult to be assented to. But, on the other side, if we consider the advantageous circumstances for its promotion, and speeding forward, and the small impediments for the hindering of these carriages had, beyond any other, we shall find much less reason to doubt the history of it: for, if we compare it with vessels sailing upon the sea, we shall find that this carriage has first a plain, hard and even surface of the shoar to pass over, without any rub or impediment; so that it is moved in a plain without rising or falling, without any unequal impediment, save only some small matter in the rubbing of the ends of the axes in the naves of the wheels, which, being well oiled, will be very little; whereas a ship at sea, when there blows a stiff gale (which is absolutely necessary, when much speed is desired) is first clogg'd in its motion by the lentor and difficulty of yielding in the medium of water; by the unequal stoppings of the rising waves, which create an undulating and unsteady motion upwards and downwards, as well as sideways; besides the slope falling and sliding away to leeward, which must be allow'd for in all side-winds, by steering some point nearer the wind, than the direct way; and consequently the length, passed by the vessel, will be as much longer than the direct distance, as the secant of such an angle is than the radius. On the other side, if we compare its motion with that of a carriage drawn by horses, or other living creatures, it plainly appears

that these were moved by an unwearied strength, whereas the horses were not long able to hold that pace. So that upon the whole, it seems to be the swiftest carriage yet known, for so great a burthen, and so long a way.

But the great objection against this invention is, that it is hardly practicable in any other place, and even there but at certain times, which possibly have been the reasons, why it has been so long disused, and almost forgotten. To which I answer, that scarce any other invention for carriage is practicable in all places: land carriage cannot be practised at sea, nor sea carriage by land; carts and coaches cannot be used in some places, by reason of the inconvenience of the ways, as in *Cornwall*. But this invention, I conceive, is not to be thought confined only to the smooth sands on the sea shoar; for I doubt not, but that if trial were made (as I hope that it will shortly be) it might be much more practicable upon the plain downs of *England*, than where it was used, by reason they are much more exposed to the wind, and also much more hard, so that the wheels need not be of so great a breadth. I conceive farther, that the carriage may be improved much in its lightness, and also in the easiness of moving. If such a chariot were made for *Salisbury Plains*, *Banstead Downs*, *Winchester Downs*, *Newmarket Row*, or some such smooth plains, and the wheels, (which need be but three) were moved upon small steel *pevots* or *gudgeeons*, in *bell-metal sockets*, well oiled, instead of being moved upon the large end of an *axle-tree*. Next, if instead of 4 wheels, 3 only were made use of, placed in the form of a triangle, the steering wheel being that which went foremost, and the place of the mast in the center of the triangle, the weight carried, to be all placed behind the mast, to which I would also have added a contrivance to retard and stop its motion, whenever there shall be occasion, which is easily to be done; somewhat after the same manner as windmills are stay'd, when there is need. By such a contrivance, I doubt not, but a chariot may be made to out-run even the swiftest race-horse, especially where the course is long and plain; and with a side wind may be carried back again to the place from whence it set out; and both forward and backward may be carried with as great a swiftness, even as the wind moves, which will not be

unpleasant to such as have suitable conveniences near their habitations; with which may be tried as many experiments of sailing near a wind, as can be tried upon the sea; the contrivance of the wheels making the motion as easy, as the water of the sea or rivers in others; and to a very swift motion, having much less of impediment, especially if the wheels be order'd to the best advantage, all manner of rubbing or sliding being thereby taken off, and even the inequality of the ways themselves may be in a great measure removed. I have been the more particular in describing this carriage, because it was the swiftest that has possibly yet been made, and therefore, on this occasion, deserved more than a transient mention, tho' I do not look upon it as an invention of the highest perfection, for this effect; but may be as much exceeded, as that exceeded a man that leisurely walks. Who it was that first invented the wheel, is not recorded in history, it having been long before any history extant (except that of the Bible) and the first mention we find of it there, is *Pharaoh's Chariot*, in which *Joseph* was exalted to ride: of whose form we know nothing but the name, tho' it had, in probability, been known long before that time; which, notwithstanding, long preceded any heathen writings now extant.

Hyginus relates, in his 2d book, where he treats *De Ophiucho*, that *Ceres* invented an one-wheel'd chariot, which *Triptolemus* (whose nurse she was) first made use of, for to make speed, to inform the world of her bounty. *Ceres cum sua beneficia largiretur hominibus, Triptolemum cujus ipsa fuerat nutrix (qui primus hominum una rota dicitur usus ne cursum moraretur) jussit omnium nationum agros circumeuntem semine partiri. In Glossis Isiodori, vehiculum unius rotæ*, is called *Pabo*. But how this one-wheel'd chariot was contrived, or used, is not to be found in history; mention there is, of other chariots, with more wheels, in the ancient authors; so that 'tis clear, it was known and practised long before any histories of heathen writers were publish'd. An invention of so great use, that it seems impossible ever to be lost by mankind, after it be once known: which consideration makes me very much wonder whence those men came, that inhabited *America*, before the *Spaniards* over-running and conquering of it; since it seems probable, that if they, or their ancestors, had

sprung from any people here, on this side of the world, *viz.* from *Europe, Asia* or *Africa*, they must needs have carried along with them the useful invention of the wheel; but it has been observed, that they knew nothing at all concerning it, nor any the least use of it, throughout all *America*, before the *Europeans* came among them. So that we must conclude, either that they were made inhabitants before the invention of the wheels was found, or that they never had any origination from any generation of men in those parts of the world, at least not from the *Tartars*, who, of all people, do most frequently use them; but this by the by. The first, and most simple of *carriages* by land, was this invention with one wheel, and may possibly be most accommodate, for attaining the end we are now inquiring after, which is swiftness, it having the least impediment to its motion, and the least incumberance of any other; and may therefore, in the next place, deserve to be considered, and possibly be brought into common use, at least to be experimented, as was that of the sailing chariot.

But before I come to the more particular description thereof, I think it will not be impertinent to examine the contrivance of the wheel, as it is applied to carriages, for the facilitating of their motion. One of the greatest obstructions to swiftness of motion being the inequality of the ways, and the rubbing or grating of those ways against those bodies or weights, that are drawn or slid upon them.

The wheel being then a round body, and moving forwards, only by its rollings, doth not at all rub, grate, or slide upon the way; and so hath no impediment at all to its motion forward, where the way is even, plain and horizontal, or level, there being no impediment, or very little, from the medium of the air it passes through, and so hath no impediment to be moved with the swiftest motion, like that of the resistence of water to vessels moving through it: so that the only impediment seems to be that of its own bulk, (of which I shall speak hereafter) because the outward rim of the wheel, in its rolling motion, doth uniformly apply its parts to the parts of the plain, by descending down, and rising up from them perpendicularly; and the touching part is always quiescent upon the plain, and moves not either forward

or backward; and consequently all impediment from rubbing upon the ground or way is wholly taken off, as 'twill be evident to any one who shall examine the motion of any one point of the verge of the wheel; for he will find that every point of this verge doth, by the compounding the circular and progressive motions together, move itself in a true *cycloidal* line, and that, in the point of touching it, resteth or standeth still in the boundary between two such lines. So that where the plain and the wheel is perfectly hard and smooth, the wheel receives no impediment to its compounded motion; but it may be thought that the circular motion of the wheel is an impediment to the progressive motion, because by means of this composition, the parts of the wheel do, in several positions therein, receive several degrees of progressive motion, and so seem to go, as it were, by starts, for that the points, whereby they touch, have no progressive motion at all; and when they are at the top, or at the greatest distance from the plain, they have a double velocity forwards, compared to that of the center, and, in every intermediate position, a differing degree of velocity forward. But this is no impediment at all to the progressive motion of the whole, each motion being severally uniform, equal, and continued. For a *pendulum*, whose weight at the end is a globe of lead, or any other ponderous body, suspended by a string, receives the same *impetus* from the power of gravity, (which is the same in both cases) whether this globe, so suspended, be suffer'd to vibrate, whilst it be swiftly whirled round upon its center, or whether it be not so whirled at all, the compounding of motions not at all intermeddling with one another; but every one keeping its distinct *impetus*, as may be easily found by experiment, if trial be made in the way I propose. Whence I conceive also, that the periodical motion of the earth, or any other planet about the sun, would be the same, whether the body of any of them were gyrated round their own centers, or not, and whether the *axis* of that gyration were at right angles with the plain, in which they are mov'd or not, the motion or influence of the one not at all interfering, or disturbing that of the other. But this only by the by. However, I think it may be pertinent to be consider'd in the examination of an *hypothesis* of *gravity*, propounded by the learned Dr. *Vossius*, in his lately

publish'd *Miscellaneous Treatise*, wherein he lays great stress upon the position of the *axis*, in respect of the plain of its circular, or direct motion.

Next, we are to consider, what impediment to its motion, a wheel, thus roll'd upon a floor, receives from that floor. There may be two impediments then, that a wheel, so roll'd, may receive from a floor according to the nature thereof. The first and chiefest, is the yielding, or opening of that floor, by the weight of the wheel so rolling and pressing; and the second, is the sticking and adhering of the parts of it to the wheel; to which two may be referr'd all others, all of which proceed from the yielding or giving way of the parts of the floor, and the not returning again to their bended posture; for, if the floor be perfectly hard (as also the parts of the wheel) tho' it be very unequal, yet is there little or no loss, or considerable impediment to be accounted for; for whatever force is lost, in raising or making a wheel pass over a rub, is gain'd again by the wheel's descending from that rub, in the same nature as a ship on the sea is promoted by the descending down of a wave, as much as impeded by its ascending, or a *pendulum* is promoted by its descent, as much as impeded by its ascent.

Nor is the yielding of the floor any impediment, if it returns and rises against the wheel, for the same reason; but the yielding, or sinking of the floor, and its not returning again, is the great impediment from the floor; for so much of motion is lost thereby, as there is force requisite to sink such a rut into the said floor by any other means; whether by weight, pressure or thrusting directly down, or any ways obliquely.

And it may also be calculated, by drawing on the wheel, whose weight, at the mean time, sinks the floor it rolls over. Either way it will be easy to bring it under calculation, which is the design of this discourse.

The second impediment it receives from a floor, or way, is the sticking and adhering of the parts of the way to it; for by that means, there is a new force requisite to pull it off, or raise the hinder part of the wheel from the floor, or way, to which it sticks, which is most considerable in moist clayie ways, and in a broad rimm'd wheel. For in such ways, the wheel doth not only

lose a part of its motion, by the yielding and pressing of the clay against the fore parts of the wheel, but by the cleaving to, and holding of it to the hinder parts, which makes all carriages move very sluggishly and heavily in such ways.

Thus much I thought necessary to consider, as to the goodness or badness of the floor, or ways over which carriages are to pass, whereof, in the general, this may be affirm'd, that the harder the ways are, the less impediment they give to the motion of carriages over them; and the more even they are, the more equal is the motion.

Hitherto I have consider'd the wheel only as free, and, of itself, burthen'd only by its own weight. I shall next consider it as burthen'd by another weight. There are two ways then of burthening a wheel. The first is, by laying the weight at the top of it; the second is, by laying it upon the center, or axis of it.

The first way was possibly the first invented, being of great use for transporting of very great weights some short way, and is generally practised for removing of obelisks, columns, great stones, or great beams of timber; and, for that use, the rollers, or wheels, are generally solid pieces of hard timber, cut or turn'd round; and are very long or broad, call'd rollers; this, of all ways, is the easiest for removing such weights; but then they must be continually chang'd by being remov'd from behind the weight, and plac'd before; for as they roll forwards upon the floor, so they roll backwards under the weight, or rather promote the same with a double velocity to that of their own upon the floor. By the way, it seems very strange, that the *West-Indians,* tho' in their buildings they made use of such vast stones, and dragged them on the ground for so great a distance, yet that they should not understand the use of these wheels, or rollers, which, histories say, they did not, they performing those transportations only, by the main strength of men pulling at the ends of a great number of ropes. By this way, a vast weight may be moved by a very small strength, if all things be hard and smooth, approaching much to the moving of a bulk upon the water; but this being more proper to be inlarged upon under the head of strength, and not so adapted for speed, I shall leave at present, till I speak of that part.

The second way then of burthening wheels, is, by resting such weight upon the axis, or center of them; this may be, and has been practised also two ways; that is, either first, by making the wheel move round upon the axis fixed to the carriage; or, secondly, by fixing the axis to the wheel, and making the axis to turn round in a socket of the carriage; the first of these ways is now, and has always been the way of using wheels for chariots, carts, waggons, and such other kinds of carriages; the second, is used in wheel-barrows, and such other carriages and uses, where the wheel runs within the frame. Of these two ways, the last (where it can be applied) is much the best; for that the axis can be much better fixed in the wheel, so as to make it run true in a plain; and next, for that the axis may be kept more firm and steady to that motion, by having the two ends of the axis, by means of its gudgeons, kept in the sockets fitted for it; and thirdly, because the gudgeons, halving the weight, may be made very much smaller, and so will not cause a tenth part of the friction which is necessary in the other way. This second way, therefore, is much better accommodated for speed than the former, and may also be well enough contriv'd, to be made applicable to several sorts of carriages fit for that purpose, of which I shall hereafter speak.

The next thing to be consider'd, is the make of the wheel itself; which has been several ways contriv'd, and made use of in differing ages of the world, and for differing occasions. The first and most simple, was that which was made of a round piece of timber for rollers, as I noted before, in which there seemed to be little of art, but only sawing it off with a saw; these were of the smallest sort, and are still used for truckles and smaller carriages.

The second, was that of a somewhat bigger sort, and that was either cut out of a whole plank, where it cou'd be procur'd broad enough, or else was made of two or more planks join'd together, and fasten'd by two or more, cross ledges, and that was call'd *tympanum*, and the same is still used for the carriages of guns at sea. The third way, was of bending a piece of pliable timber, as we now do for hoops, and thereby making the rim of the wheel all of one piece, and fixing the spokes to it, which were also fix'd into a nave in the middle; which nave was also turn'd and bor'd, as the naves, we now use, are.

The last, and most practicable of all, was that we now use, whereof the rim was made with several fellows join'd and yok'd together with pins, and sometimes with joints, and strengthen'd also by the sides with irons, and, after all, bound round with iron streaks and nails. This way is used for all sort of carriages, whether heavier or lighter; and wheels, thus made, are differenced only by being made either bigger or less in compass, or stronger and weaker in substance or bulk; whence they become also thicker or thinner, in breadth or thickness, and also heavier or lighter, according to the various designs and uses they are apply'd unto; the circumstances and accidents, that concomitate their design'd use, best directing the artist in the contrivance of their form and make.

I shall not now insist upon explaining, which sort is most proper for every of these designs, because I shall do that under each proper head; but shall only consider at present, which kind of these are best for speed and celerity, that being the head I am now explaining.

For making of speed then, those sorts of wheels are best which are the biggest in circumference or diameter, because first, a much greater part of the rim doth bear at once, than in a wheel of a less circumference; for the way being always more or less yielding, the bigger wheel sinks in so much less to come to its bearing, than the lesser wheel, by how much the greater circle approaches nearer to a streight line, or the tangent of the floor. Secondly, because the greater the arch, the more easy is the rise of the wheel over any irregularity, or rub in the way, and the easier the fall, and thereby approaches nearer to the evening and plaining of the way, and makes less inequality in the draught. On the contrary, the smaller the wheel, the worse, for that it introduces all the contrary inconveniences. Thirdly, the larger the wheel is in circumference, the less is the impediment of the rubbing and wearing.

For *first*, the leaver of the spoke is so much the longer, and so the nave will turn so much the easier upon the end of the axle; the weight born, in both cases, being the same, and consequently the bigness, both of the one and the other, needing not to be differing.

Secondly, the lighter the wheel be (provided it be made strong enough to perform the business it is design'd for) the better it is; and therefore all manner of contrivance that tends to the making the wheel strong, and yet large and light, is to be made use of, for that thereby a less weight is necessary to be moved, and consequently the same strength will have the greater effect.

Thirdly, the less rubbing there be of the axle, the better it is for this effect; upon which account, steel axes, and bell-metal sockets, are much better than wood, clamped, or shod with iron; and gudgeons of hardened steel, running in bell-metal sockets, yet much better, if there be provision made to keep out dust and dirt, and constantly to supply and feed them with oil, to keep them from eating one another; but the best way of all is, to make the gudgeons run on large truckles, which wholly prevents gnawing, rubbing, and fretting.

These are some of the good qualifications of wheels, prepared and adapted for the design of speed, which I am now discoursing of: there are some other qualifications that yet exceed these, of which I shall treat some other time, where I shall have occasion to apply them.

Having thus far consider'd of the properties and qualifications of wheels, fit for such carriage, I shall next consider what kind of carriage is best for this purpose, and what number of wheels are fittest to be applied.

First, for the properties of the carriage. That which is of the smallest bulk, and of the lightest weight, and of the simplest, plainest, and yet strongest and most durable structure, is the best; provided still, that, in every particular, it be sufficient for performing what is required of it. That carriage, which is only design'd for carrying a single man, should not be made either large enough, or strong enough, or heavy enough, to carry two; that, which can be born by one or two wheels, should not be loaden, or clogged, with two, three, or four. So that upon the whole matter of the instrument, fit for conveyance of one single person, I see none can be better than a certain carriage or chariot, and for the convenient reception of one man, and resting or moving upon one single wheel. I do not find this to be in practice any where, but in *China*, of which there is a short account in

Martinius his *Atlas Sinicus.* But this is not so well adapted for swiftness, being moved by the strength of men, and, for the most part, by one, and so is only a chair, or sedan, with one man and a wheel, instead of a second man; but might be contrived much better, both for ease and speed, if there were two men made use of with one single wheel, which I shall elsewhere describe; but still it will come short, as to speed, in comparison to one, wherein the strength of horses, or some such swift and powerful mover, is applied for its acceleration.

The next thing then to be considered, in an engine for speed, is the application of strength for the moving thereof, which is the life of the whole; and without which, all the rest is motionless. This I shall discourse of the next time.[1]

Stevinus's Sailing Chariot.

Mar. 2. Mr. HOOKE read a paper about SIMON STEVIN'S sailing chariot and other sorts of motion.[2]

Light after Sunset in February.

Mar. 11. A letter of Monsieur JUSTEL was read, mentioning a light seen after sunset.

Mr. HOOKE remarked, that the same thing had been mentioned by Mr. CHILDREY in his *Britannia Baconiaca*; and that it often appeared about the beginning of February.

Sugar Trees.

Mar. 18. Mr. HOOKE said, that there were several sacchariferous trees mentioned by PISO and some other writers.

1685

(32) *De Dioptris Telescopicis.*

In Hevelius *Annus Climactericus*, 1685.
A Latin translation of an English MS. by Hooke.

Barometer High before a Frost.

May 13. Mr. HOOKE observed, that some time in a winter before a frost the quicksilver of the barometer has been higher than at any other time of the year.

Ruby Glass.

May 27. Mr. HOOKE observed, that the scarlet-red window-

[1] I do not find any account, among Dr. *Hook's* Papers, of the matters here promised. WILLIAM DERHAM.

[2] It is printed in the *Philosophical Experiments and Observations*, p. 150.

glass was brought from Germany; but that there had been none brought over for eighty years past.

Hackney Waterwork.

June 2. HOOKE showed a plan of a Waterwork at Hackney newly finished by Mr. ALDERSEY. [R. S. MS., No. 71.]

Zodiacal Light.

June 3. Mr. HOOKE read an account of a luminous phenomenon,[1] in colour like the tail of a comet, seen sometimes after sunset in the west, and at other times in the east before the sunrising, lying under or near the ecliptic, reaching from about forty degrees from the sun to about seventy, being fourteen degrees large, and ending in a point. It had been first mentioned by Mr. CHILDREY in a book printed about 1661, but seen by him several years before, and since that time observed by Mr. HOOKE; but in 1683 published as a new discovery by Monsieur CASSINI at Paris, and since by Monsieur FATIO at Geneva.

Samuel Watson's Astronomical Watch.

June 10. Mr. HOOKE supposed, that it might be the same piece of watch-work, that was in the King's bed-chamber; and was of opinion, that it would not reach the exactness, that was pretended.

Zodiacal Light.

Mr. HOOKE made a further explanation of his opinion about the luminous phenomenon mentioned at the last meeting, illustrating it with several schemes.

Logarithmic Instrument.

June 17. Mr. HOOKE showed an instrument for the drawing the logarithm line, which, he said, was supposed by DESCARTES not to be practicable.

June 25. *Lectures on Longitude.*[2]

Spiral Drawing.

July 15. Mr. HOOKE showed some further use of his instrument for describing spiral lines; which he was desired to print, or, to give a copy to be entered in the society's books.

Zaffora.

Dr. SLARE gave an account of the mineral 'Zaffera'. Possibly HOOKE's paper 'Of the mineral called by Dr. LISTER 'Zaffora' may be referred to this period. [R. S. MS., No. 95.]

[1] *Posthumous Works*, pp. 193–200.

[2] loc. cit., pp. 510–18.

Cobalt Glass.

July 22. Upon reading the minutes of the last meeting Mr. HOOKE produced a transcript of a passage in Mr. KUNCKEL's book *De Arte Vitraria,* which he had procured a translation of; agreeing with the account, that Dr. SLARE had given of cobalt and zaffer, and confirming what his experiments had made out.

Chinese Writing and Abacus.

Mr. HOOKE read a discourse relating to the Chinese character, and their way of casting account,[1] which he compared to, and illustrated by the ancient Roman abacus.

He was ordered to procure a Roman and a Chinese abacus to be made, and to be kept in the repository.

July 29. Mr. HOOKE brought in a model of the Roman and Chinese abacus, which he had caused to be made for the repository, and continued his discourse about the Chinese language.

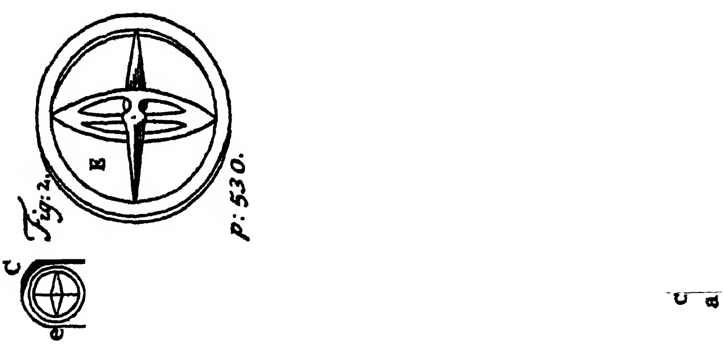
Sept. 25. Note concerning the *Rumb-line projected from the Pole.*[2]

Oct. HOOKE reviewed HEVELIUS's *Annus Climactericus,* wherein he vindicated the justness of his Celestial Observations against the exceptions by some made to the accuracy of them.

Phil. Trans. No. 175, p. 1162.

Nov. 12. *Lecture on The ways of finding Longitude.*[3]

Nov. 19. *Lecture on Practical Geometry.*[4] This paper mentions an



instrument for drawing spirals or Rhumbs, which WALLER has described as follows:

Let there be made a thin flat ruler *abc,* having a slit through the

[1] Printed in the *Philosophical Transactions,* vol. xvi, No. 180, p. 63, for March and April, 1686.

[2] *Posthumous Works,* p. 542.

[3] loc. cit., p. 518.

[4] loc. cit., pp. 523–30.

middle of it to slip freely upon a centre pin fixt perpendicular in a plane upon which the line is to be drawn. At the end of this ruler *c* there must be cut a round hole with a ring of brass exactly fitted to it, and in this ring in its socket in the ruler, the plane of the wheel may be set to any angle with the slit in the ruler; which done by moving the ruler upon the plane round the Centre, the edge of the said Truckle-wheel will describe upon the plane, the spiral with the angle required at every revolution proportionally approaching the fixt centre. E is the Truckle-wheel and ring taken out of its socket in the ruler.

Norris's Book.

Dec. 2. Mr. HOOKE read a paper of observations upon Mr. NORRIS's book, wherein he declared, that he had formerly shown a way for describing the rhumb line and some other things relating to navigation: and that he had perused Dr. WALLIS's observations upon Mr. NORRIS's book.[1]

Standard Weight.

Mention being made of a universal measure, Mr. HOOKE said, that he had thought of a natural body, which might be a universal standard, which he would discover at the next meeting.

n. d. About this period HOOKE drew up a Report on *Hydrography*. See p. 690.

1685/6

Feb. 2. *Letter from* S. BARRON *to* HOSKINS *and* HOOKE re description of Tonqueen. [MS. Sloane, 1039, f. 133, and R. S. Mss.]

Halley to report on Society's Papers.

Feb. 3. That Mr. EDMUND HALLEY, as clerk, do inquire and satisfy the council what inventories of the society's papers were made, when they were taken into the care of Mr. HOOKE and Mr. ASTON:

That he bring in an account of what he finds wanting of the society's books and papers; and that he inquire after them.

Barometer.

Mr. HOOKE brought in an invention of his for nicely discovering the alterations of the height of the barometer by obviating the objections against Mr. HUBIN's way, of the different counterpressure.[2]

[1] R. S. MS., No. 68.

[2] *His Experiments and Observations for the Improvement of the Barometer*, read this day before the society, are printed in his *Philosophical Experiments*, &c., p. 169.

Experiments and observations for the improvement of the Barometer, by Dr. HOOK, *read before the Royal Society.* [Derham, p. 169.]

The experiments I have now shewn, are no ways pompous and surprising. Such possibly may better suit a stage or theatre, for vulgar spectators to admire and gaze at, who are most taken with shew. But these are plain and obvious, and only valuable, as they discover some truth, that may be either useful of itself to be known, or has a tendency to the making some farther discovery, or of being useful, as preparatory to some other experiment or invention, which may be made or founded thereupon. And indeed the greatest part of experiments, if they be not made for some such design; and the material circumstances, useful thereunto, diligently enquired after, and strictly observ'd, and brought to a calculation for that purpose, do serve for little else than to hint an experiment to some other to try, who may have some use or application for it.

The experiments, as they have been made, do exhibit the specifick weight of the fluid bodies; together with their comparative weight with water: that these three fluids are in specifick gravity to one another, as follows.

Water, 5997.
So water to *mercury*, as 1 to 15.

Spirit of wine, 5102.
Oil of *turpentine* to *mercury*, as 1 to 17¼.

Oil of *turpentine*, 5209.
Spirit of wine to *mercury*, as 1 to 17.

Further observables are,

First, the great lightness of spirit of wine, and oil of *turpentine*, they being, spirit of wine but as 51. Oil of *turpentine*, 52, whereas common water is 60; that is, almost a sixth part lighter than water.

Secondly, the nearness of their specifick gravity to one another, which may be yet made as much nearer, as shall be requisite, or desired, by the intermingling water, or flegm, with the spirit of wine; for the spirit of wine being lighter, and the oil of turpentine heavier, some mixture of water, with the spirit of wine, will

bring the spirit of wine to be as near of the same weight, with the oil of turpentine, as shall be required.

Thirdly, the differing nature of these so seemingly similar liquors.

First, in that they will not mix with each other, but will float the one upon the other.

Secondly, in that they will not easily receive the same tincture, but differing; the spirit of wine readily imbibing a red, from cocheneel, which that, and the spirit of turpentine, a green.

The use, or application of these experiments, is in order to the solution of this following mechanical *problem*.

How to make a *barometer*, or instrument, to try and find the weight of the air, at all times, which shall rise and fall steadily, and without jumping or starting, otherwise than as influenced by the air, and the hitherto unknown alterations thereof; whose limits, between the greatest and the least height, shall be 10, 20, 30, 40, 50, or more feet in perpendicular; and the motion, in every inch of the said height, as plainly visible, as the rising and falling of an inch in the common single *barometer*.

It is about 7 or 8 years since I propounded such a *barometer* to this Society; and I cannot expect that many such will be made; however, possibly it might not be amiss, that this Society, or some curious observing person, would make one, and diligently remark the changes and motions thereof. For it might possibly discover such changes and motions of the air, as we have hitherto no notion or conjecture of; for I did once observe, that the wheel-*barometer*, a little before a great storm of thunder, lightening and rain, did appear to have a tremulous motion, as if the room, or post it hung upon, had shook, when yet the clouds were but gathering, and were far enough off from this place, where I observed it; of which I have, long since, acquainted this society, and, I conceive, it may be found in the Journal. But there are many other changes in the air, that none of the instruments, we yet have, will detect; and therefore there may be scope enough for inventions, of other kinds, to detect them, which may give a farther light to the discovery of that most significant, and most useful, body of the air. And tho' possibly the invention of a mechanical instrument may be looked upon as a trivial thing,

yet, as it may be contrived and applied, it may furnish us with a new sense, by which we may be able to know some properties of bodies, of which we have no more notion, than one born blind has of colours, or one deaf of musical sounds; or than the whole world hath ever had, of the differing gravitation of the air, before the *barometer* was invented and observed.

The reason of my contriving this instrument, was, that I might shew a way how the examination, or weighing of the air's pressure, might be carried to the extreams, or as far as could well be desired; for so it may be, by this method, if any one will be at the charge of making it.

And indeed if we consider, and a little more strictly examine into the nature of things, we shall find, that most of the operations of nature are out of the reach of our senses, and cannot be plainly, if at all, discover'd by them, and we are left to guess at the consultations and designs of the Privy Council of Nature, only by the publick acts and effects that are produced thereby; whereas, if we could by sense be informed of the agents, and of the method or way of acting, used by those agents, we should be much better able to give a right judgment of the effects.

Now there is no method of information so certain and infallible, as that of sense, if rightly and judiciously made use of. And though the senses themselves are limited in their power and extent, when considered barely in themselves, as naturally constituted, yet their power may be much enlarged, and their limits much farther extended, by the helps that art may afford, and, most especially, by mechanicks; by means of which, not only each of them may be made more powerful in the discovery of the proper objects of those several senses; but each of them may be made a *genus*, as it were, of new sorts of sense, comprised under them, of which we have yet no notion, nor any sense or method of discovery; at least they are yet unheeded. I might instance, in the body of the air itself, but I shall reserve it to another opportunity.

In air,	13℥, ½ʒ.
In water,	½ʒ, *gr*. 83.
In spirit of wine,	2½ʒ, 28 *gr*.
In spirit of *turpentine*	2℥, 2ʒ, 41 *gr*.

Air 105½.

Water	5½, 3 *gr.* ——	1003 — 3 *gr.*
Spirit of wine	20, 28 *gr.* ——	853 + 2 *gr.*
Ole. tereb.	18½, 11 *gr.* ——	863 + 4 *gr.*

Wherefore I find that spirit of wine may easily be made to be 16 times lighter than mercury; if then the spirit of wine be made of this specifick weight, by intermingling water with it, and the height of the pipes, or the cylinder of spirit of wine be designed to play 32 foot perpendicular; then must the mercurial be 2 foot more in height, than the common *barometer*; which I have found sometimes (as particularly on *Wednesday* last) to be 30·6; and consequently the mercurial cylinder to counterpoise the gravity of the air, and the gravity of a cylinder of 32 foot in height of spirit of wine, of such a rectification as I have specified. Now, the cylinder of the spirit of wine being always the same, that is, 32 foot, the counterpoise to it of mercury will be always the same 2 foot; and the cylinder of the air only altering the cylinder of the mercury also, that counterbalances that also, will only be alter'd, and that the same, as in the common *barometer*. Now if the oil of turpentine be $\frac{1}{96}$ part lighter than that, then a cylinder of mercury $\frac{1}{96}$ shorter than two foot, will counterpoise it; which is but one quarter of an inch difference in the counterpoising cylinders.

Telescopic Sights.

Mr. Hooke mentioned also another experiment, to show the advantages of telescopic sights over plain ones.

He was desired to explain both these at the next meeting.

Inventory of Papers.

Feb. 10. Mr. Halley reported from Mr. Hooke, that there was no particular inventory of the society's papers taken, when they were delivered into his custody.

Conservation of Minerals.

Mr. Hooke remarked, that the best way to preserve minerals, whereof the quantity was but very little, was by glueing them down to cards to be stuck to the bottom of a box.

Mr. Hooke said, that these brown earths are a tincture given by water running through some iron mineral.

Mr. Hooke mentioned, that he had in his custody some English cobalt, which is the mineral, out of which blue smalt is

made. He was desired to produce it to the society, which he promised.

Telescope Sights.

Mr. HOOKE was desired to show his experiment about the comparison of plain and telescopic sights at the beginning of the next meeting, it being now too dark to see it with the exactness requisite.

Dr. Pell's Papers.

He reported, that the papers of the learned Dr. PELL, lately deceased, were partly in custody of Dr. BUSBY, and the rest at Brereton in Cheshire. He was desired to do his endeavour to obtain the said papers for the society.

Barometer.

Mr. HOOKE read his discourse of the improvement of the barometer by taking off the inequality of the pressure of the coloured liquor in Mr. HUBIN's way.

He was desired to give it in to be registered; but he desired to be excused at that time, promising to bring in an account thereof shortly.[1]

High Barometer.

He mentioned upon this occasion, that on Wednesday the 3rd instant the quicksilver was exceedingly high, viz. 30·6 inches; and that he had never observed it higher:

That once a year in the winter months of December, January or February the quicksilver stands higher than in all the rest of the year besides, generally presaging frost.

32-foot Glass Tube.

Mr. HOOKE asserting it, it was queried by Dr. LISTER if the Register or Journal of the society mentioned a glass cane of thirty-two feet long made for the Torricellian experiment.

Density of Air.

Mr. HOOKE mentioned an instrument of his contrivance, whereby he could discover the thickness or thinness of the air, abstracted from its gravity; which he was desired to perfect and bring in.

Standard Weight.

He was desired to proceed to show his experiment, whereby he would establish a general weight and measure by means of

[1] It is inserted in the Register, vol. vi, p. 293, and is printed in the *Philosophical Transactions*, No. 185, p. 241, for November and December, 1686.

a drop of quicksilver; it being queried by some of the members, whether the 500th part of a grain could, by any means, be discovered in a scale.

Telescope Sights.

Feb. 17. Mr. HOOKE showed his experiment to prove the excellency of telescopic sights above plain ones, by comparing a direction by the naked eye with a radius of ten feet, with that of a telescope of eight inches; and it appeared to the satisfaction of all present, that there was a very great advantage in the telescope.

Scales on Small Instruments.

He promised to bring in at the next meeting an invention of his, whereby the limb of a very small instrument may be divided so as to have as many discernible parts, as a very large one, according to the usual manner of the division of instruments:[1] as likewise another contrivance to set such a small instrument perpendicularly to take altitudes as well as by a larger radius.

Barometer.

He affirmed, that Mr. HUBIN's contrivance of the barometer was formerly brought in to the society by himself, and that he in truth was the inventor thereof.

Mr. HALLEY was ordered to search the books of the society, to see, whether there be any record thereof.

Density of Air.

Mr. HOOKE promised again to show an instrument, that should distinguish the gravity of the ambient air.

Universal Standard.

He being called upon for the experiment, whereby he would make a universal measure and weight from a drop of quicksilver, desired to be excused for some time till the sun had gotten more northward.

Boyle on Motion.

Mr. HOOKE read and gave in an account of Mr. BOYLE's book on *Languid and unheeded Motion*, presented at the last meeting.

Roman Brick.

Mr. HOOKE presented a Roman brick taken out of London wall near Aldgate; the dimensions whereof were 11 inches in breadth, 17 in length, and 1½ in thickness.

Feb. 24. Upon occasion of the brick, presented at the last

[1] Register, vi, p. 293.

meeting by Mr. HOOKE, Dr. GALE remarked, that those large bricks were such as VITRUVIUS called *diatoni*; and that their use was to bind the wall together, being generally the whole thickness thereof.

Hooke versus Hevelius.

Mr. HOOKE read a paper vindicating himself from some injuries, which he conceived done him by Mr. HEVELIUS in his *Annus Climactericus.*

Adjustment of Small Instrument.

Mr. HOOKE promised to bring in at the next meeting his contrivance for setting a small instrument perpendicular as exact as a larger.

Density of Liquids.

He likewise showed a glass cane bent at one end, to show the proportional weight of mercury to the liquors.

Experiments.

Mar. 3. Mr. HOOKE being called in was desired to bring in a scheme of a method for making experiments; which he said he would consider of against the next meeting.

Diving.

Mr. HOOKE gave an account of conveying air under water for the use of diving by a chain of buckets inverted, which he had formerly brought in February 17, 1663/4: And it was ordered, that it should be seen what account there is of this experiment in the Register.

Adjustment of Small Instrument.

Mr. HOOKE showed his way of setting a small instrument perpendicular by means of a triangular pendulum; which he was desired to bring in an account of in writing.

Scheme for Experiments.

Mar. 10. It being again desired of Mr. HOOKE to give in his scheme of a method for his experiments, he requested, that the society would nominate a subject for experiments to be made by him; but that they would leave him to his own method of prosecuting them. Whereupon the council desired him to proceed upon which of the said eighteen heads applicable to navigation he should think fit; and ordered him to have a copy thereof.

Diving.

Upon search of the Register Book of the society, it appeared, that Mr. HOOKE's way of carrying air under water for the use of divers by a chain of buckets inverted, brought in by him in the year 1663, was not entered there.

Ignition of Pyrites by Water.

A paper of Mr. WORLIDGE occasioned much discourse concerning the cause of fountains; and Dr. ROBINSON was of opinion, that the streams raised by a subterraneous heat, either of fire or a fermentation within the bowels of the earth, was the most probable cause of springs; which yet was opposed by some others of the members.

Mr. HOOKE remarked, that the stone called brass lumps or pyrites would catch fire by moisture; and that a heap of coals was by this means fired at Charing Stairs.

Temperature of Air in Deep Well.

Mr. HOOKE having made several observations on the temperature of the air at above 300 feet deep in a well on Banstead Downs in 1665, it was ordered to see what account there was of that experiment in the books of the society.

Adjustment of Small Instrument.

Mr. HOOKE brought in his account of the manner of setting a small instrument perpendicular; but it being late, the reading of it was referred to the next meeting.

Mar. 17. Mr. HOOKE showed his demonstration of his manner of setting a small instrument perpendicular, and was again desired to bring in an account thereof in writing at the next meeting, with its application.

Smokeless Engine.

Mar. 24. Mr. HOOKE was of opinion, that the engine, which is said to consume smoke, contained in it the fire of spirit of wine, or oil of turpentine, or the like: which flame concealed in the cavity of the engine, and drawing its air by the hearth or furnace, the smoke of anything, that was laid on the furnace, was thereby carried inwards to the flame of the burning liquor, which consumed it, or at least it dissipated its parts, so as it was no further sensible.

Analysis of Hydrography.

Mr. HOOKE, at the desire of the council, brought in an analysis of the whole matter of hydrography, of which he had given a fair

copy to the President, and promised to give another to the society.

of
Bodleian Library, with a Pepysian paper, in MS. Rawlinson A 171. It is dated 23 March, 1685/6.]

Wax for Roman Ships.

Dr. GALE remarked, that the Romans never used either pitch or tar in their shipping, but wax.

Mr. HOOKE observed, that he remembered to have read somewhere, that they mixed with their wax the dregs of oil called *amurca*; but that he could not at that time recollect his author.

Adjustment of Instrument.

Mr. HOOKE further illustrated his contrivance for setting a small instrument perpendicular. He was desired to procure his instrument to be made for the use of the society.

1686

Detonation of Gunpowder.

Mar. 31. Mr. HOOKE gave an account of his firing gunpowder *in vacuo* with a burning-glass; and said, that now and then a single corn would go off upon the whole heap without kindling the next corn; and that at length having melted the heap into a lump, it went off after the manner of the pulvis fulminans with a very great report, and burst his glass into a thousand pieces, and stuck great part thereof into the ceiling.

Cuttle-fish Beaks.

Mr. HALLEY brought in two shells or substances resembling small beaks of birds found in ambergris.

Mr. HOOKE thought them to be the shells of some such insects, as the *Scarabaeus nasicornis*.

April.

(33) *Some observations and conjectures concerning the Chinese characters.*

Phil. Trans., No. 180, p. 63, April 1686.

Density of Mercury and Water.

Apr. 14. Mr. HOOKE showed an experiment for finding the comparative weight of quicksilver to water by a siphon filled with mercury in the one shank and water in the other. The water was observed 79⅝ inches above the mercury, which was

counterposed by $5\frac{7}{8}$ inches of mercury standing above the section of water and mercury in the other shank, whence the proportion of their gravities is as 47 to 635, or as 1 to $13\frac{1}{2}$.

W. Molyneux on Hooke.

Apr. 21. A letter from Mr. WILLIAM MOLYNEUX to Mr. HALLEY, dated at Dublin, April 8, 1686, was read, as follows:[1]

'Mr. HOOKE's contrivance for the baroscope is admirably curious. I suppose what you mention as Monsieur HUBIN's invention is rather Monsieur HUYGENS's invention, and described in the *Journal des Sçavans du Lundi* 12 *Decembre* 1672. But Mr. HOOKE's goes beyond it in many respects: the only difficulty of Mr. HOOKE's is in the fabric of the glass, and filling it with mercury; and if he have any mechanical easy contrivance for doing this, he would do well to publish it. I thank you for the promise you make me of his small, yet accurate level. Indeed I have always had a great esteem of his mechanical inventions, of which I look upon him to be as great a master as any in the world; and that is a most curious part of philosophy, and really useful in man's life. And whereas, I understand, that a chance word in an idle scribble of mine was something displeasing to that ingenious and learned gentleman, I desire he may be informed, that I designed no manner of slight by the word *pamphlet*, but styled his book so, merely as I thought it a name usually given to small stitched volumes.'

Density of Water and Turpentine.

Mr. HOOKE showed the experiment of the equilibrium of water and oil of turpentine in an inverted siphon, as he had done before of mercury and water; and it appeared, that $92\frac{1}{1}\frac{5}{6}$ of water was equal in weight to $107\frac{7}{16}$ inches of oil of turpentine; whence the weight of water to that of oil of turpentine is as 1,719 to 1,487, or as 1 to 0,865. Hence the weight of mercury to oil of turpentine is as 15 to 62 or as 4 to 1.

Ancient Painting Media.

May 19. Upon mentioning, that no metallic colours were used in painting in fresco, it was queried about what time the several sorts of painting came to be in use: To which Mr. HOOKE answered, that the most ancient painting was with gums; then with white of egg; then with wax; and lastly with oil; the particular time of the beginning of each not being easily attainable.

[1] Letter Book, vol. x, p. 292. Mr. HALLEY in his letter, Supplement to Letter Book, vol. iv, p. 329, to which this of Mr. MOLYNEUX is an answer, mentioned, that Mr. ASTON and Dr. ROBINSON had *without any apparent cause* resigned their office of Secretaries.

Level.

Mr. HOOKE read a paper vindicating his level from some objections made against it from Dublin: wherein he supposed, that the pendulum thereof was not made with the accuracy necessary; and in the end he proposed a further use of that invention by applying the pendulum thereof to the pendulum of a clock, which being capable of being made to vibrate as slow as you please, would make the clock go much slower than ordinarily used, so as to make the time of the winding up but very seldom, with the same number of teeth, which the ordinary clocks have.

Publication of Newton's Principia.

It was ordered, that Mr. NEWTON's *Philosophiae naturalis principia mathematica* be printed forthwith in quarto in a fair letter; and that a letter be written to him to signify the society's resolution, and to desire his opinion as to the print, volume, cuts, &c.[1]

May 25.

(34) *Two Astronomical Observations of the Eclipses of the planet Jupiter by the Moon in March and April last made at London.*

Phil. Trans., No. 181, pp. 85–7, May 25, 1686.

Refractive Index of Ice.

May 26. Mr. HOOKE likewise occasionally mentioned that ice is not only lighter than water, but has a refraction considerably different therefrom.

[1] Mr. HALLEY wrote accordingly to Mr. NEWTON on the 22nd of May the following letter, Supplement to Letter Books, vol. iv, p. 340.

'There is one thing more, that I ought to inform you of, viz. that Mr. HOOKE has some pretensions upon the invention of the rule of decrease of gravity being reciprocally as the squares of the distances from the centre. He says you had the notion from him, though he owns the demonstration of the curves generated thereby to be wholly your own. How much of this is so, you know best; as likewise what you have to do in this matter. Only Mr. HOOKE seems to expect you should make some mention of him in the preface, which it is possible you may see reason to prefix. I must beg your pardon, that it is I, that send you this ungrateful account; but I thought it my duty to let you know it, that so you might act accordingly, being in myself fully satisfied, that nothing but the greatest candour imaginable is to be expected from a person, who has of all men the least need to borrow reputation. I am, &c.'

With regard to this claim of Mr. HOOKE, the reader may see the letters of Mr. NEWTON and Mr. HALLEY in the article of the former in the *General Dictionary historical and critical*, vol. vii.

Pendulum and Level.

Mr. Hooke read a discourse about the application of the pendulum of his level to a clock to make the vibrations thereof as slow as required; and he showed the manner of applying it to a watch for regulating its balance.

Salary.

June 2. It was ordered, that the Council Book be searched as to what had been done about Mr. Hooke's salary.

Fire-engines.

A note from Monsieur Justel was read about an engine used at Amsterdam for quenching fire: whereupon Mr. Hooke gave a full account of an engine, which he conceived to be the same, being made with a gut or pipe of canvas to convey the water through crooked passages, where our sort of engines cannot come to play.

Mr. Hooke showed the draft and contrivance of a water-engine at Hackney made by one Mr. Aldersey, wherein three pumps are moved by an axis with a triple crank by means of an overshot wheel.

Squaring the Circle.

June 9. A foreign gentleman, subscribing his name Georgius Rash, sent in a letter, enclosing, as he conceived, the solution of a problem, whereby the exact quadrature of a circle might be easily found. It was by an algebraical method of inquiring into the properties of a curve called by him *isobole*, because it has its ordinates increasing in the same proportion as the angles made at a point in the axis. Mr. Hooke having perused it said, that the curve here proposed as new was no other than the *linea quadratrix*, being an old thought of those, who had attempted squaring the circle. See Clavius.

£60 to be paid.

June 16. It was ordered, that Mr. Hooke be allowed his arrears for the years 1684 and 1685; and that the Treasurer pay him sixty pounds in full till Lady Day last.

Molyneux on the Level.

June 30. A letter of Mr. William Molyneux to Mr. Halley, dated at Dublin, June 19, 1686, was read, containing some remarks on Mr. Hooke's level and barometer.

Chinese Writing.

Mr. Hooke read a discourse concerning the nature of the

Chinese character, of their books, numbers, and writing, which he conceived to have been the literal character of some ancient language now lost, so that the figure remaining, and not the sound or *potestas* thereof, it is become a real character, but encumbered with so much difficulty, that there is scarce any other help but memory. He was desired to publish this discourse, according to his intention, in a *Philosophical Transaction*.[1]

July 7. Mr. HOOKE's notion of the Chinese language and characters occasioned much discourse about the *potestates* of the letters of the ancient languages; some being of opinion, that their sound was continued down to us; and Mr. HOOKE seeming to maintain the contrary.

Variation of Magnetic Needle.

Mr. HOOKE showed a contrivance for nicely observing the variations of the magnetical needle; which he promised to prosecute against the next meeting.

[The accompanying discourse has been printed with his *Posthumous Works*, p. 484.]

Seamless Shirt.

July 14. A shirt of fine calico without any seam either in the body or sleeves was produced by Mr. HOOKE, who having well considered it, offered at some conjectures about the manner of weaving it; which he promised to communicate in writing.

Indian Lac.

July 28. Mr. HOOKE said, that true Indian lac or varnish is the gum of a tree; and that it was usually mixed with an oil; and that it is, when green, of a very poisonous nature, and would make the flesh swell where it had touched the skin.

Chinese Lac.

Aug. 4. Upon reading the minutes of the last meeting, wherein it had been proposed, that Chinese lac might be a good preservative for ships, Mr. HOOKE said, that upon inquiry he had found, that the Chinese junks are varnished with lac only above water: and he observed, that the fabric of those junks is without ribs, the planks only laced with bamboos, and so plastered.

Molyneux on the Level.

A letter of Mr. WILLIAM MOLYNEUX to Mr. HALLEY, dated

[1] It is printed in the *Philosophical Transactions*, No. 180, p. 63, for March and April, 1686.

Discourse on Petrified Shells on Tops of Hills.

Mr. Hooke read a further discourse of his, by way of introduction to a theory of his concerning the petrified shells and such-like substances found in the bowels of the earth, and on the tops of hills.

Discourse on Petrifactions.

Dec. 22. Mr. Hooke read a continuation of his discourse concerning shells, &c. wherein he gave several material instances to prove, that there have been very great changes in the earth's surface, as of rows of oyster-shells found in a cliff in the Alps, sea-sand and shells at a great depth in St. James's fields, and the like shells observed by himself at a great height from the sea in a cliff in the Isle of Wight.

Pistol Experiment.

Dec. 29. Mr. Hooke proposed, that it might be tried whether or no the ramenta of steel struck off in the experiment of the last meeting of the pistol *in vacuo* be melted and vitrified, as is usually observed *in aere*, as may be seen in Mr. Hooke's *Micrographia*. It was ordered, that Dr. Papin prepare this experiment against the next meeting.

Mr. Hooke said, that steel filings being cast through the flame of a candle are thereby melted or calcined; and that those, which make the most vivid sparks, catch fire and flame so as to ascend thereby: and he showed this to be so by experiment.

Alcohol used to drive Air out of a Vessel.

He mentioned the experiment of driving out all the air in a vessel by the vapours of spirits of wine.

Tides in Arctic Ocean.

Mr. Hooke remarked, that he had been credibly informed, that the tide of flood comes out of the east into a second strait more easterly than that of Weiggats; and consequently, that Nova Zembla is an island, and that there is a great ocean to the east thereof instead of the imaginary Tartaria magna.

Ammonites and Nautilus.

Mr. Hooke read a further continuation of his discourse about shells, wherein he considered the structure of the nautilus and cornu ammonis stone, tending to prove, that though it be true, that there is no animal known, resembling in all points the lineaments of those lately produced by himself; yet that it is not a sufficient argument to evince, that there is not nor ever was any such animal *in rerum naturâ*.

1686/7

Inspection of Halley's Books.

Jan. 5. It was ordered, that Mr. WALLER, Mr. HOOKE, Mr. PITFIELD, and the two Secretaries, or any two or more of them, be a committee to inspect the books of the society, to see if Mr. HALLEY had performed his duty in relation to the entries to be made by him, according to an order of council of January 27, 1685/6: and

Work and Payment.

That Mr. HOOKE bring in against the next meeting of the council a proposal in writing of what he is willing to perform for the society; and what he expects by way of gratuity from them: which he promised to do; and a meeting of the council was ordered to be summoned for this day sevennight to consider of his proposal.

Nautilus.

In order to prove, that the incredible bigness of the nautilus or cornu ammonis stone is no argument, that there have not been such shell-fish of that magnitude, Mr. HOOKE produced a quotation out of MANDELSLO'S *Travels*, wherein mention is made of an oyster, the shells of which weighed above 400 lb.; which shells were then in the Duke of HOLSTEIN'S collection of rarities.

Hooke's Offer to the Society and the Society's Offer to Hooke.

Jan. 12. Mr. HOOKE made a proposal, that he would produce one or two experiments and a discourse at every meeting, provided his salary be made up 100 *l. per ann.*

Hereupon, after much debate, it was concluded, that Mr. HOOKE should have 50 *l.* a year from the society, and their lawful assistance and recommendations towards his recovery of the 50 *l.* a year, which Sir JOHN CUTLER stood obliged to pay him during his life: and that in consideration thereof Mr. HOOKE should at every meeting produce one or two new experiments, together with a discourse concerning them in writing, to be left with the Secretary: and that the said experiments should proceed in a natural method.

Postponement of other Business.

The time being far spent, Mr. HOOKE'S lecture and experiment, as also those of Dr. PAPIN, were ordered to be referred to the next meeting.

Twilight.

Jan. 19. The Bishop of Soissons in his letter to Monsieur JUSTEL mentioned, that he had lately observed the crepusculum or time of twilight to be of a much longer continuance than usual, and desired to know, if the same had been observed here. In answer to which Mr. HOOKE said, that he had lately seen a glade of light in the morning such as is common in the evening in March and April; but that he had never observed the like before at this time of the year.

Dyeing.

Mr. HOOKE described a method for dyeing several colours on the same piece of cloth in panels or squares, which he conceived to be the way used by the Indians to stain their calicoes.

The Earth's Changes.

Mr. HOOKE read a further lecture concerning the changes, that seem to have happened in the earth's surface, and proposed three queries, viz: 1. Whether the earth's poles are fixed in the earth, or not? 2. Whether the earth's surface be truly spherical? and 3. Whether all perpendiculars pass exactly through the same point or centre?

Pistol Experiment.

The experiment was tried of the ramenta of steel struck off by the fall of the cock of a pistol *in vacuo*, to see, whether they would be melted into small hollow globules, as they are *in aere*: and it was found, that they were only exceedingly thin plates of steel, which had not suffered the least liquefaction, as could be discovered on several of them.

Lecture on the Earth and its Fossils.

Jan. 26. Mr. HOOKE read a lecture, giving his hypothesis, how shells and such-like substances come to be found deep in the earth, and far above the surface of the sea, as it is at present. He supposed, that the diurnal rotation of the earth by its *vis centrifuga* taking off part of the gravity formed the surface of the sea into a compressed spheroid; that is, that the diameter by the poles is the shortest, and those of the equinoctial greatest, which some experiments of the shortening the pendulum near the equator seem to make out. Then, if it may be supposed, that the poles and axis are movable, the equinoctial and greatest diameter will be likewise altered, and by consequence the parts of the land, towards which the poles approach, will be raised, and the sea retire; but, on the contrary, those parts, from which the poles recede, will sink, and the water rise upon them: and that the poles may be altered, he endeavoured to prove by

alleging the latitudes of several places considerably different from those assigned by PTOLEMY and the old geographers.

[*Posthumous Works*, pp. 343–4.]

Feb. 2. Mr. HOOKE read a further discourse concerning the probability of the hypothesis, that the earth is of the figure of a prolate spheroid, whose shortest diameter is the axis. He alleged several instances of assertions concerning the elliptic figure of the planets, as of the sun by SCHEINER, of Jupiter by CASSINI, and of Mercury by GALLET. He then proposed several experiments proper to examine this hypothesis, as by the vibrations of a pendulum-clock in places near the poles and near the equator; as also by trying, whether a degree of latitude be exactly equal in all parts of the world. Then he proceeded to inquire, whether the axis be fixed in the earth, or not; and among other queries, whether the vast sandy deserts of Africa and Arabia owe not their original to the sea? and whether there remain any antique buildings, wherein the true meridian should have been designed, such as the Egyptian pyramids, the Athenian temple of the winds, &c.? He concluded with a promise to produce at the next meeting a method capable of determining by observation the question of the mobility of the axis in a few years. To make it probable he alleged an experiment tried by himself, viz. that a ball being turned on its axis, as it swims on mercury, would change the axis of the rotation.

[Printed *in extenso* in *Posthumous Works*, pp. 350–4.]

History of Fishes.

Feb. 9. It was ordered, that Mr. HOOKE be desired to write to Amsterdam to a bookseller of his acquaintance there about the disposal of the *History of Fishes*; and that he agree for 400 books at 25*s*. a book, whereof two-fifths to be paid in money, the rest in exchange of such other books, as shall be thought requisite for the society's library.

Figure of the Earth.

Mr. HOOKE produced a demonstration of the spheroidal figure of the earth, proceeding from the complication of a gravity or descent towards the centre (which he supposed every way equal at the same distance *a centro*) and a *conatus a centro* or *vis centrifuga*, occasioned by the diurnal rotation of the earth, which is always in a line perpendicular to the axis, and proportioned to the confine of the latitude.

True Meridian and Method for discovering a Change in the Earth's Axis.

He then proposed a method of finding nicely the true meridian

line by means of a long telescope, which he explained by showing how to be certain of the direction of such a long telescope; and then how to derive the horizontal base-line from the hypotenuse given: by which exact observation he concluded it possible to determine, whether there were any change in the earth's axis more surely than by the coarse observations of the ancients; and that if any such change be, it might, even in a few years, be by this means discovered. [*Posthumous Works*, pp. 355–60.]

The True Meridian.

Feb. 23. Mr. HOOKE showed his manner of discovering the true meridian, by adapting a telescope so, as the direction of the sight therein should exactly answer to a line on the outside of the tube. Then directing this telescope towards a small telescopic constellation, called by him the English Rose (which, he said, he had discovered just about the present pole-point, and wherein he formerly had marked the very point) and then letting fall two perpendiculars from the side of the tube, he concluded, that the true meridian would be most exactly designed; this method having the advantage of being [able] to be put in practice at all times of the night, when clear, and these small stars to be seen with a two-foot glass. [*Posthumous Works*, pp. 361–2.]

Mar. 9. The experiment of finding the meridian line by the means of the telescopical stars very near the pole was by reason of the cloudy weather deferred till the next meeting.

The Atmosphere.

Mr. HOOKE read a lecture concerning the figure of the atmosphere, which he conceived to be of a shape much more oval than the water; both because gravity is weaker under the equator than under the poles, whereby the air becomes more expanded there than here; as likewise upon the account of heat and cold, the said difference is still more considerable. From the prosecution of this notion he promised to explain the causes of several phenomena seeming of great difficulty, particularly those of the trade-winds. [*Posthumous Works*, pp. 363–4.]

Shifting of the Poles.

Mar. 23. Upon reading the minutes of the last meeting, it was the opinion of the members now present, that the protrusion of mountains by subterraneous fire or otherwise may occasion some alteration of the poles of the earth, as well as the accession of new matter.

Purple Dye from Mussel.

Mr. HOOKE produced a book entitled *New England's Rarities*, printed at London, 1672, in 8vo. In this, p. 37, mention is made of a scarlet mussel in that country at a place called Paschataway about fifty leagues east from Boston, where in a cove called Bakers Cove is a sort of mussel with a purple vein, used in that place to mark shirts, handkerchiefs, and other linen.

1687

To find the Latitude.

Apr. 6. Mr. HOOKE showed a method of finding the latitude of places by help of a planisphere of the fixed stars, drawn after the gnomonic projection. It was by finding two stars in the same azimuth, and two others in some other azimuth about ninety degrees from the former, at the same instant of time. Then drawing lines on the planisphere through each pair of those stars respectively, the common intersection of those lines will show the latitude of the place by the declination of the point of intersection on the planisphere. He promised to show at the next meeting how this method might be made more general by solving this problem: two stars come on a certain azimuth, and after a given space of time two other stars come on some other azimuth: the latitude of the place of observation is required.

Apr. 13. After the reading of the minutes Mr. HOOKE showed a construction of the problem of finding the latitude of a place by the help of two azimuths of two stars and the interval of time between them; which being too intricate to be understood upon reading, he was desired to give it in at the next meeting in writing.

Grains said to have Fallen in Rain in Wiltshire.

Mr. HOOKE remarked, that formerly such grains seeming to have fallen in rain had been inquired into, and found to be no other than ivy berries: and such he supposed these to be.

The Latitude.

Apr. 20. Upon reading the minutes of the last meeting, Mr. HOOKE intimated a method, whereby he could more easily find the latitude of a place by observation of two azimuths of twice two stars and the interval of time between, which he showed to be general as well in the stereographic as the gnomonic projection. It consisted in changing the right ascensions of one pair of the stars as much as the heavens move in the time between

the two observations, and then proceeding as if the two azimuths had been observed at the same moment of time.

Reflecting Telescope to discover the True Pole Point.

Mr. Hooke showed a reflecting telescope made to take in several degrees. This he proposed as a very proper instrument to discover the true pole-point among the telescopical fixed stars.

Acorn Cups used for Dyeing.

Apr. 27. Mr. Houghton presented some very large acorn cups, brought out of Syria, and called by the Italians *valanie.* They were said to be used in Venice, both acorns and cups, by the dyers to dye black.

Mr. Hooke remarked, that he had seen such a large sort of acorns, brought out of Barbary near Tangier.

Latitude.

Mr. Hooke showed again the scheme of the construction of the problem of finding the latitude by azimuths of twice two stars.

Dr. Hook's *ways to find expeditiously and certainly, the true meridian; being somewhat different from the method in his* Posthumous Works, *page* 361. [Derham, p. 206.]

Provide a short telescope of 1 foot, or 18 inches in length, fitted with a glass plate in the focus; upon which proper circles must be drawn, with the point of a diamond, for the Pole Star, and two other stars not far distant from the pole, which is supposed to be in the center of this glass. This telescope must be fitted with two plumb-lines. Now by this instrument, in any fair night, tho' the moon shine, it will be very easy to discover the proper stars, thro' this telescope, and to see that each of them be in its proper circle, about the polar point: at which time, the axis of the glass will be in the true meridian, and, if fitted with the quadrant, give the altitude; and the plumb-lines being in the meridian, there may be a compass suspended by them, which will also shew the variation easily and certainly. This instrument is sufficiently intelligible, without any scheme, which is therefore omitted.

Another way is wholly new, and the observations are made without an instrument, and the refractions of the air do no ways influence either the observations or deductions. And that is, by

observing, with plumb-lines, or other proper instruments, either both at the same time, if it may be, or one at one time, and the other at another, with a true account of the interposed time, two azimuth lines, in each of which are found two considerable stars. By the help of which two observations, and a true projection of the sphere of the stars, it will be easy and obvious, to any navigator, to find the latitude of the place, the meridian line, and the azimuths of the stars.

These two ways were proposed to the Royal Society, April 27, 1687. W. DERHAM.

[An 'Answer to WALLIS's better way to find the Meridian'. R. S. MS., No. 75.]

May 11. Mr. HOOKE read a further discourse[1] concerning his manner of finding the latitudes of places by the azimuth of twice two stars.

Discourse on Vegetation.

May 18. Mr. HOOKE read a discourse concerning vegetation grounded on the experiments and observations of Mr. BROTHERTON, and explaining his three propositions given in at the last meeting. Mr. HOOKE promised to insert this discourse in the *Philosophical Transactions*.[2]

May 25. Upon reading the minutes of the last meeting, Mr. BROTHERTON's experiments occasioned much discourse about vegetation and grafting.

Mr. HOOKE remarked, that Mr. BROTHERTON had cut on the bark of a fir-tree, which in time healed up again; and that the bark and tree throve, though all the direct fibres of the bark were divided; which seemed to argue insertions or anastomoses in the vessels of the bark, whereby the sap descends.

Mr. HOOKE remarked, that Mr. BROTHERTON had observed, that seeds suspended in the air no sooner had emitted their germ out of the husk but it immediately turned upwards against the perpendicular.

Rotation of Earth.

Mr. HOOKE read a discourse[3] concerning a suspicion of his, that the earth being made up of heterogeneous parts may have some inequality in the diurnal rotation from the different actions of the sun and moon; and he proposed an experiment to try it,

[1] Printed in *Posthumous Works*, pp. 543–4.
[2] No. 187, p. 307.
[3] Printed in *Posthumous Works*, p. 545.

by observing the interval of time between the passages of two stars having the same declination through a fixed telescope at several times of the same night. In the same discourse he supposed the reason of the moon's keeping one face always towards the earth to be, because this hither side of the moon is heavier than any other, and thereby gravitating most towards the earth, that part is always, except a small oscillatory motion, turned towards us.

Coffee Roasting.

Mr. Hooke supposed, that the roasting of coffee is a sort of malting thereof to make it give its tincture; and that without roasting it would not make coffee.

Exact Time Measurement.

June 8. Mr. Hooke read a discourse concerning a method of finding the interval of time to the utmost exactness, by showing how to divide the time of each vibration of a pendulum into its parts: and he showed the model of an instrument for the doing thereof. This he conceived might suffice to examine the query, which he moved at the last meeting, about the unequal rotation of the earth.

Micrographia Nova.

Mr. Hooke produced a book, entitled *Micrographia nova*, published by one Griendelius of Nuremberg; giving the figures of several insects, seeds, &c. many the same with those in Mr. Hooke's *Micrographia*, but much worse designed.

Hooke's Salary to take Precedence.

June 15. The question being put, whether the order for the payment of Mr. Hooke's salary, made June 16, 1686, should be executed before any other orders made since either for salaries or gratuities, upon balloting, it was carried in the affirmative.

Exact Time Measurement.

June 22. Mr. Hooke read a further lecture concerning the exact measure of time, and showed the demonstration of the division of the arch of oscillation of a pendulum so as to show equal times: which is done by dividing the arch in the proportion of sines, and taking the whole length of the arch vibrated for diameter.

Burning Speculum and its Composition.

Mr. Hooke read a relation of the extraordinary force of a burning concave speculum out of the *Acta eruditorum Lipsiensia* for January 1687, p. 52. He supposed, that if such a speculum

were made of many feet diameter, the effects thereof might be expected most prodigious. He proposed, that such a one might be made of copper tinned with a mixture of tin, lead, and isinglass, which might be made very large for a small price, and bear a very good polish.

June 25. *Discourse on the Minute division of Time.*[1]

Latitude.

June 29. Mr. HOOKE read a discourse[2] concerning a method of finding the latitude of a place by observing two equal altitudes of a star passing near the zenith, having found two points, the one perpendicularly under the other, and the exact time betwixt them.

Payment of Salary in 'Fishes'.

July 6. The question being put, whether Mr. HOOKE should have the arrears, due to him by a former order of June 16, 1686, paid him in like manner in copies of the *History of Fishes*, it was balloted and allowed: only Mr. HOOKE desired six months' time to consider of the acceptance of such payment.

Transmission of Force.

Mr. HOOKE read a discourse concerning the way of conveying force to a great distance, which he conceived would best be done by some stiff and inflexible rod, as a wire, or long pole, or the like; and showed the experiment by communicating a force given in the inner hall of Gresham College across the quadrangle by means of a packthread, which was found to perform to satisfaction.

Crystals of Salt.

July 13. Mr. HOOKE showed in a microscope the crystals of the salt presented at the meeting of June 29 from Dr. CHARLETON, said to have been congealed like sugar-candy on a stick in a river of Siberia. The particles thereof were shot into square grains like table diamonds, which looked very fair in the microscope, so that it appeared to be a sort of *sal gem* or marine salt.

Mr. HOOKE's discourse, by reason it was now late, was referred to the next meeting.

Anatomy of Eye.

July 20. Mr. HOOKE related, that Mr. WALLER and himself having lately dissected several eyes, had observed a great number of small threads or nerves entering through the sclerotis into

[1] *Posthumous Works*, p. 549.
[2] *Posthumous Works*, p. 550.

the cavity of the eye towards the bottom, and proceeding between the sclerotis and uvea, to terminate in the outward ring of the *Processus ciliares*, so to serve for the motion of the crystalline humour, and also to make the aperture of the uvea bigger or less.

Transmission of Motion by Rods.

Mr. HOOKE likewise read an answer to Dr. PAPIN'S objection to the communication of motion at a distance by rods, with a further explication of the vibration of the rods or poles: as also another discourse, wherein he further explained the great inconvenience of Dr. PAPIN'S way, and the impracticability thereof, by showing: 1. That it would be next to impossible to make pipes to hold so perfectly, as not to leak air in some parts. 2. That it would be as difficult to discover one or more such leaks, or, when found, to stop them. 3. That neither his gutters nor turpentine nor molasses would prevent those difficulties; for that all such a gutter must be uncovered two leagues, when such leaks happen; next, the gutters would not do uphill and downhill, nor crossroads: and they would be as difficult to be kept tight from leaking out those substances; nor would they hinder evaporation. And though it were possible, yet: 4. That the air is the worst of all *media* for conveying such power, there being more strength lost thereby than by any solid medium or fluid, as water, &c. because of the great springiness thereof.

Vibration of Long Rods.

Mr. HOOKE showed the experiment of vibration of the rods, as a pendulum, which was by suspending a large India cane of about thirty feet long by two packthreads about eight feet in length: by which it was plain how the weight of such rods or poles for communication of traction or pulsion at a distance might not only be made to move freely and with ease, but also be in the nature of the weight of a sway.

The same thing was also tried with a large scaffold-pole of above forty feet long, suspended by two small cords, which succeeded, as the former to the satisfaction of those present.

[At this meeting HOOKE appears to have also read a paper 'Of raising water by rarefaction. Imperfection of Dr. PAPIN'S engine for raising water: a remonstrance touching some of his discoveries'. 3 pp.] [R. S. MS., No. 76.]

Seeing in the Dark, and Light in Eyes of Cat.

July 27. Upon reading of the minutes of the last meeting, it was inquired, whether there were any emission of light from the eyes of cats to help them to see in the dark, as in glow-worms, fire-flies, and the like: but it was the opinion of the members

present, that in cats, owls, and such like animals, the extraordinary faculty of seeing in the dark arises from the great dilatation of the pupil of the eye.

Mr. HOOKE remarked, that the light seen in the eyes of cats is rarely found but when the cat is frightened; or else very earnest after her prey.

Nature of Fossils.

Oct. 26. Mr. HOOKE read a letter to himself from Mr. WALLER concerning stones, as nautili and ophiomorphites, lately found by him near Cainesham Bridge in Gloucestershire.[1] One of these stones was evidently formed in the shell of a common nautilus, but much bigger than the usual sort; and not only the diaphragms were most distinct, but also the holes in them, whereby the several cavities communicate, were indisputably discovered, and no room left to doubt of its having been once a shell.

Hanno on Great Changes in the Earth.

Nov. 2. Mr. HOOKE read a discourse upon the *Periplus* of HANNO, wherein are several things seeming to favour his notion of the great changes, that have happened in the earth; as where mention is made of countries flaming in the night, and a high mountain expiring fire; the description of which seemed to agree with the Canary Isles, which Mr. HOOKE conceived to have been blown up by fire.

Trisection of an Angle.

Nov. 9. Mr. HOOKE gave his report of Monsieur TARRAGON's book of the trisection of an angle, which had been recommended to him at the last meeting, viz. that this construction was not general, and only extended to the trisection of such angles, whose third parts were the $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, &c. of some other angle had by the bisection of an angle.

£60.

Nov. 23. The President signed an order for the payment of sixty pounds to Mr. HOOKE, which had been ordered him on the 16th of June, 1686.

Boyle's Final Causes.

Mr. HOOKE gave an account of Mr. BOYLE's book of *Final Causes*, wherein by a great many instances he endeavours to show the unreasonableness of those, who deny design in the proceedings of nature. The second part of the book being an account of several accidents befalling the eyes, Mr. HOOKE desired, that it might be perused by some physician of the society.

[1] Keynsham in Somersetshire.

£22 10s.

Nov. 29. It was ordered, that Mr. HOOKE be paid 22*l.* 10*s.* for three-quarters of a year's salary due to him at Christmas last.

Lecture on Figure of the Earth.

Dec. 1. After the minutes Mr. HOOKE read a lecture, being first a recapitulation of several things, which he had produced before the society the last year, particularly concerning the spheroidical figure of the earth, the mutation of the poles, and the consequences thereof.[1] He then cited a passage out of PLATO's *Timaeus*, seeming to make much for his hypothesis of the frequency of floods and conflagrations. It was the relation of an old man to PLATO's grandfather, who had it from an Egyptian priest, that the island of Atlantis was once so considerable, as to have inhabitants, who had conquered good part of Africa and Europe; but that in one day's time the whole island sunk into the sea. Lastly, he gave a translation of HANNO's *Periplus*, from some passages whereof he collected, that there was then a conflagration of some lands, which, as he interpreted the words κατ' εὖθυ, were in the latitude of 36 degrees, and, as he conceived north-west from Madeira: and he supposed the mountain called Θεῶν ὄχημα in this *Periplus* to be the peak of Tenerife; asserting, that he had good grounds for it; and that the *caldera* or basin on the top of it is warm with a subterraneous heat.

Dr. GALE remarked, that κατ' εὖθυ would bear the sense, which Mr. HOOKE put upon those words; but he supposed this *Periplus* to be of a Greek rather than Carthaginian original, because all the names of places are Greek.

To this it was answered, that the Greek translator might take the liberty to change significant names into his own language.

Jessop's Propositiones hydrostaticae.

Mr. ASTON presented from the author FRANCIS JESSOP, Esq., his book lately printed, entitled, *Propositiones hydrostaticae ad illustrandum Aristarchi Samii systema destinatae*, &c. which was delivered to Mr. HOOKE to bring in an account of.

Dec. 6. TANCRED ROBINSON wrote to SLOANE: 'The Royal Society declines apace; not one correspondent in being. The revenue is settled upon Mr. HOOKE, and Monsieur PAPIN goes back next week to settle in Germany.' [MS. *Sloane* 4036, f. 30.]

[JOHN RAY had resigned the Secretaryship of the R. S. in the previous year.]

£37 10s.

Dec. 14. It was ordered, that the Treasurer pay Mr. HOOKE

[1] *Posthumous Works*, p. 377.

thirty-seven pounds ten shillings, as a further gratuity, and in full of all demands since the last order of November 29 past.

Historical Interpretation of Ancient Fables.

Mr. HOOKE read a discourse tending to show, that the ancient story couched in fable had yet a real and truly historical interpretation.[1] And after that manner he interpreted the story of PERSEUS in OVID, deriving PERSEUS from *περιζέω*, and understanding by him lightning; and the metamorphosis of ATLAS to have been the destroying the Hesperian gardens, and blowing up by a subterraneous fire the great mountain Atlas in the place where they were.

He promised to show a like mythology of several of the other fables of antiquity.

September.

(36) *A Relation of the great effects of a new sort of Burning Speculum lately made in Germany: taken from the Acta Eruditorum of the month of Jan. last: being a letter from the Inventor to the author of that Journal.*

Phil. Trans., No. 188, September 1687.

September.

(37) *Observationes nonnullae eclipseos solaris, 1 Maii 1687, ab Hookio et Halleio seorsum factae.*

Phil. Trans., No. 189, p. 370, September 1687.

1687/8

Feb. 15. *Lecture on Changes on the Earth and on Noah's Flood.* [*Posthumous Works*, pp. 403–10.]

Feb. 29. *Lecture on the Flood.* [loc. cit., pp. 410–16.]

1688

July 18. *Lecture on the Consequences and Concomitants of Earthquakes*, and the *Alterations caused by them in the constitution of the Air* as to Sickness, etc. Earthquakes in China and Spain. [*Posthumous Works*, pp. 428–33.]

[1] *Posthumous Works*, p. 377, et seq.

1689

May 29. *Lecture on the Causes of the present State of the Surface of the Earth. Answers to objections based on* 1. *Petrifactions.* 2. *Figured Spars and Chrystals.* [*Posthumous Works*, pp. 433–6.]

PLATE I. Drawings of Cornua Ammonis or Snakestones drawn in great detail by HOOKE to convince people that they must once have been alive.

PLATE II. Placent Nautil-shells for comparison with the Snake-stones figured on Plate I.

1. Nautilus in section. 2. Nautilus. 3. Argonanta or Japan Nautilus. 4. Spirula described by HOOKE as an unknown species. Helmet-stone petrifactions for comparison with recent Echini.

'Anyone that will diligently and impartially examine both the Stones and the Shells, and compare the one with the other, will, I can assure him, find greater reason to perswade him of the Truth of my position [that Fossils were once alive] than any I have yet urged or can well produce in Words; no Perswasions being more prevalent than those which these dumb Witnesses do insinuate.' [HOOKE on Earthquakes, *Posthumous Works*, p. 285.]

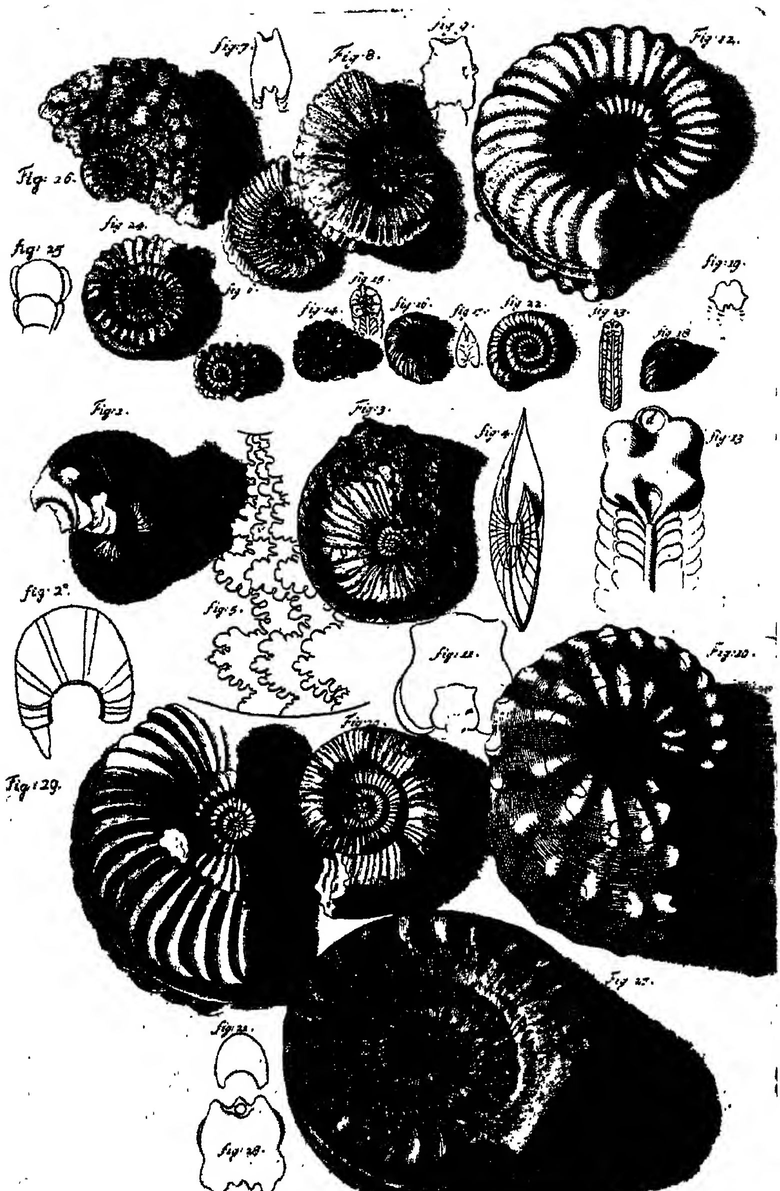
June 26.

An experiment shewn before the Royal Society by Dr. HOOK *of the penetration of dimensions in the mixture of vitriol and fair water.*[1]

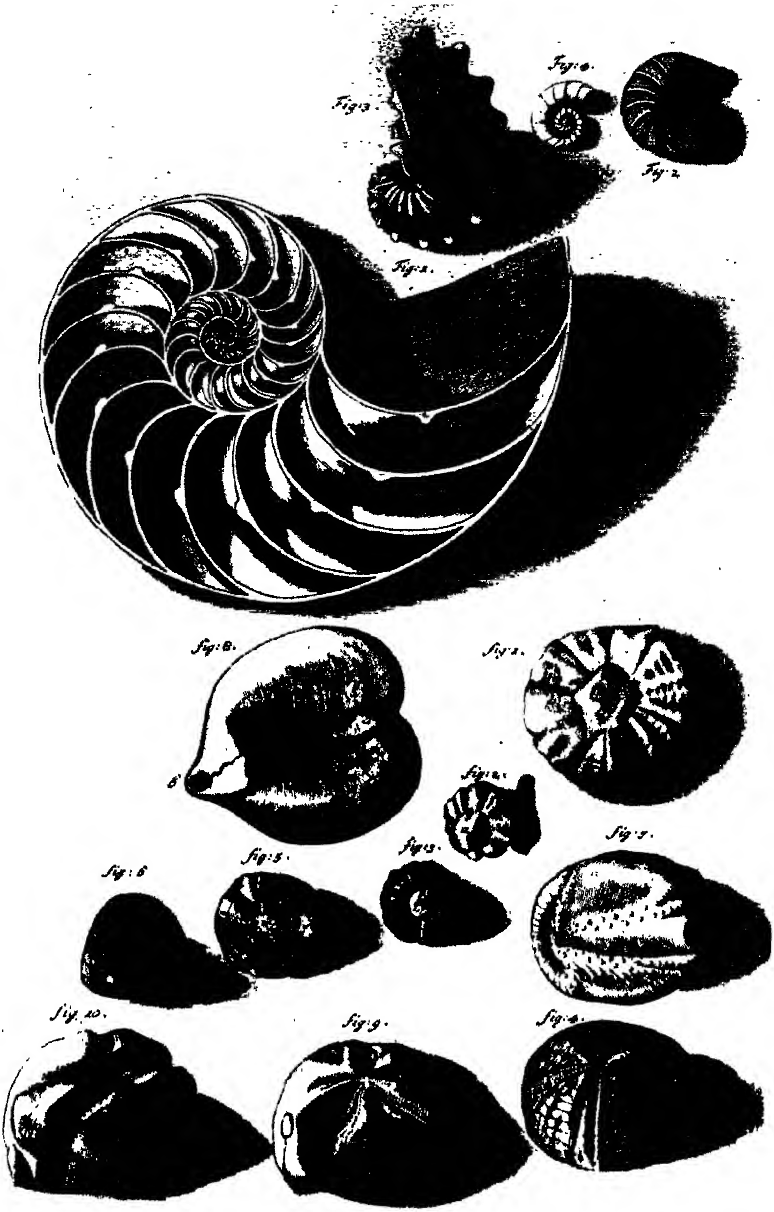
[Derham, p. 207.]

Tho' several experiments have been made of the dissolving of several differing sorts of salts, successively in the same water, after it has been satiated with one particular salt, so as to dissolve, or take into it no more of that salt; yet, in all these experiments, there seems not to be any real penetration of dimensions; nor do I know of any other experiment of the like nature, that has been made by any person. But, I conceive, it is very considerable in this, that water, which has not (by the greatest force which has been yet applied to it) been compress'd into lesser dimensions, should yet admit a thicker, closer, and more ponderous liquor to penetrate its dimensions, without any pressure or force put to assist the operation; and that two liquors, so differing in other qualities, should so readily, and harmoniously join and incorpo-

[1] Among the HOOKE MS. at the R. S. is No. 77 entitled 'The experiment of Oil of Vitriol and Water.' 3 pp., read 26 June, 1689 '(nothing in this to be printed)'.



SNAKE-STONES OR CORNUA AMMONIS DRAWN IN DETAIL BY HOOKE
FOR COMPARISON WITH RECENT NAUTIL-SHELLS



RECENT NAUTIL-SHELLS FOR COMPARISON WITH THE SNAKE-STONES
HELMET-STONES FOR COMPARISON WITH LIVING ECHINIDS

rate together. They differ first in weight; for I find that the oil vitriol, to the water, is very near as 9 to 5; they differ in the taste, the one being the greatest acid, we know, and the other perfectly insipid; the one very sluggish, and not rising in fumes, but with violent heat; the other evaporating very easily. It were too long to mention many other differing qualifications and effects; but this is worthy noting, that the mixture of those two liquors, both actually cold, produces a very strong actual heat, and thereby causes a rising of many small bubbles out of the water, and also an expansion of both, for a time, as plainly appears; for that as the mixture grows cold, so it retires and shrinks into lesser dimensions, as is visible to the eye.

Now that I might give a more exact account of the success I had, and what was likely to be expected upon another trial; here I tried the experiment with all the care I could. First then we weighed the bolt-head, and found its weight, empty, $2085\frac{1}{2}$ grains. Then we filled it almost to the top of the neck, with common water, and found its weight to be 8775 grains; from which, taking the weight of the bolt-head, we found the water to weigh $6689\frac{1}{2}$ grains; then making a mark on the neck, at the top of the water, we poured out so much as filled a small glass cane, and set a mark at the top of the remaining water, and found it 18 inches and a half below the first mark; the bolt-head, and water, now weighed 8255 grains; whence the weight of the water, taken out, was 520 grains. Then pouring off the water, in the cane, we filled it with oil of vitriol, and pouring it into the bolt-head, we found it not to fill the former space, and to make a considerable heat in the water, and many small bubbles to rise: we then weighed it again, and found the bolt-head, and mixture, to weigh 9210 grains; whence we found the weight of the oil of vitriol to be 945 grains: we let the mixture stand about half an hour, by which time we found they were so condensed, that 5 inches and half, of the 18 inches and half, of the neck, were left empty, which is near a third part of the dimensions of the oil of vitriol, that was poured therein; then we filled up the vacuity, and found it to contain 138 grains; which compared to the whole bulk of water, that fill'd the bolt-head, is between a 48th and a 49th part; for as 138 to $6689\frac{1}{2}$, so 1, to $48\frac{131}{276}$.

From which observations I deduce, that in this experiment there is somewhat more than a bare mixture of fluid with fluid, as of water with water; where tho' they may intimately mix, and temper together, and become one uniform fluid, yet each of them, and every part of each, keeps its former dimensions and specifick gravities; or of water with wine, ale, or the like inspissated liquors; or with saline solutions, as of salt, niter, allum, vitriol, *&c.* In all which, I conceive, that there is nothing but a mixing, tempering, or dilating, as in the mixture of two liquors of the same kind. Now, as I formerly hinted, I do not at all doubt, but that there may be found many other liquors which may have the like effects, one upon the other, upon mixture; so that there may be also found instances of a differing nature, where the mixture shall increase the dimensions of the particulars, and diminish the specifick gravity, either of one, or both. But I think there have not yet been produced any instances of these, or the other kind, at least, I think, they have not yet been proved such.[1]

July 24. '*Of the Penetration of Bodies.*' [R. S. MS., No. 78.]

Sept. 15, 1689. LETTER OF J. AUBREY TO A. WOOD.

Mr. Wood! Mr. Rob. Hooke, R. S. S. did in A°. 1670, write a Discourse, called, "An Attempt to prove the Motion of the Earth," which he then read to the Royal Society; but printed it in the beginning of the yeare 1674, wherein he hath delivered the theorie of explaining the cœlestial motions mechanically, his words are these, pag. 27, 28, viz.

"I shall only for the present hint that I have in some of my foregoing observations discovered some newe motions even in the Earth itself, which perhaps were not dreamt of before, which I shall hereafter more at large describe, when further tryalls have more fully confirmed and compleated these beginnings. At which time also I shall explaine a systeme of the World, differing in many particulars from any yet known, answering in all things to the comōn rules of mechanicall motions. This depends upon 3 suppositions;

"First, that all cœlestiall bodys whatsoever, have an attractive or gravitating power towards their centers, whereby they attract not only their own parts, and keep them from flying from them,

[1] Mr. *Waller* recommended the trial of this experiment to Mr. *Hawkesbee*, and if the reader hath a mind to see the success thereof, he may find it in the *Philos. Trans.* of 1711, No. 331. WILLIAM DERHAM.

as we may observe the earth to doe, but that they doe also attract all the other cœlestial bodys that are within the sphere of their activity, and consequently that not only the sun and the moon have an influence upon the body and motion of the Earth, and the Earth upon them, but that Mercury, also Venus, Mars, Saturne, and Jupiter, by their attractive powers have a considerable influence upon its motion, as in the same manner, the corresponding attractive power of the Earth hath a considerable influence upon every one of their motions also.

"The second supposition is this, that all bodys whatsoever, that are put into direct and simple motion will soe continue to move forwards in a streight line, till they are by some other effectuall powers deflected and beat into a motion describing a circle, ellipsis, or some other uncompounded curve line.

"The third supposition is, that these attractive powers are soe much the more powerfull in operating, by how much nearer the body wrought upon is to their own centers. Now what these severall degrees are, I have not yet experimentally verified (*but these degrees and proportions of the power of attraction in the cœlestiall bodys and motions, were communicated to Mr. Newton by R. Hooke, in the yeare 1678, by letters, as will plainely appear both by the coppy of the said letters, and the letters of Mr. Newton, in answer to them, which are both in the custody of the said R. H. both which also were read before the Royal Society at their publique meeting, as appears by the Journall book of the said Society*). But it is a notion which if fully prosecuted, as it ought to be, will mightily assist the astronomer, to reduce all the cœlestial motions to a certaine rule, which I doubt will never be done true without it. He that understands the natures of the circular pendulum and circular motion, will easily understand the whole ground of this principle, and will know when to find direction in nature, for the true stating thereof. This I only hint at present to such as have ability and opportunity of prosecuting this inquiry, and are not wanting of industry for observing and calculating, wishing heartily such may be found, having myself many other things in hand, which I will first compleat, and therefore cannot soe well attend to it. But this I durst promise the undertaker; that he will find all the great motions of the world to be influenced by this principle, and that the true understanding thereof, will be the true perfection of Astronomy."

About 9 or 10 years ago, Mr. Hooke writt to Mr. Isaac Newton, of Trinity coll. Cambridge, to make a Demonstration of this Theory, not telling him at first, the proportion of the gravity to the distance, nor what was the curv'd line that was thereby made. Mr. Newton, in his answer to the letter, did expresse that he had not known of it; and in his first attempt about it, he calculated

the curve by supposing the attraction to be the same at all distances: upon which, Mr. Hooke sent, in his next letter, the whole of his hypothesis, scil. that the gravitation was reciprocall to the square of the distance, which would move the motion in an ellipsis, in one of whose foci, the sun being placed, the aphelion and parhelion of the planet would be opposite to each other in the same line, which is the whole cœlestial theory, concerning which Mr. Newton hath a demonstration, not at all owning, he receiv'd the first intimation of it from Mr. Hooke. Likewise Mr. Newton hath in the same booke printed some other theories and experiments of Mr. Hooke's, as that about the oval figure of the earth and sea: without acknowledging from whom he had them, though he had not sent it up w^th^ the other parts of his booke till near a month after the theory was read to y^e^ Society by Mr. Hooke, when it served to help to answer Dr. Wallis his arguments produced in the R. S. against it.

Mr. Wood! This is the greatest discovery in nature that ever was since the world's creation. It never was so much as hinted by any man before. I know you will doe him right. I hope you may read his hand. I wish he had writt plainer, and afforded a little more paper.

Tuus,

J. AUBREY.

Before I leave this towne, I will gett of him a catalogue of what he hath wrote; and as much of his inventions as I can. But they are many hundreds; he believes not fewer than a thousand. 'Tis such a hard matter to get people to doe themselves right.

Dec. 4. *Of the Arithmetick of the Brachmans. Of a sort of Carolina Cloath.* [R. S. MS., No. 79.]

Dec. 18. *Of refitting the Weather-Clock. An account of Bangue. Observations on East Indian Voyages. Dipping Wells. Decrease of Gravity towards the Equinoctial.* [R. S. MS., No. 80.]

1689/90

Mar. 5. *A Lecture of the preference of Strait to Bunting Sails.* [*Posthumous Works*, p. 563.]

1690

July 23. *A Discourse of Earthquakes in the Leeward Islands.* [*Posthumous Works*, pp. 416–24.]

Dec. 3. *Lecture on Scientific Instruments for Navigators.* Read again on 5 and 12 Dec., 1694. [*Posthumous Works*, p. 553.]

1691

June 20. Letter from HOOKE to RICHARD LEVETT, master of the Haberdashers Co. [(Copy) MS. *Sloane* 1039, f. 131.]

Dr. HOOK'S *description of some instruments for sounding the great depths of the sea, and bringing accounts of several kinds from the bottom of it. Being the substance of some of his lectures, in December*, 1691. [Derham, p. 225.]

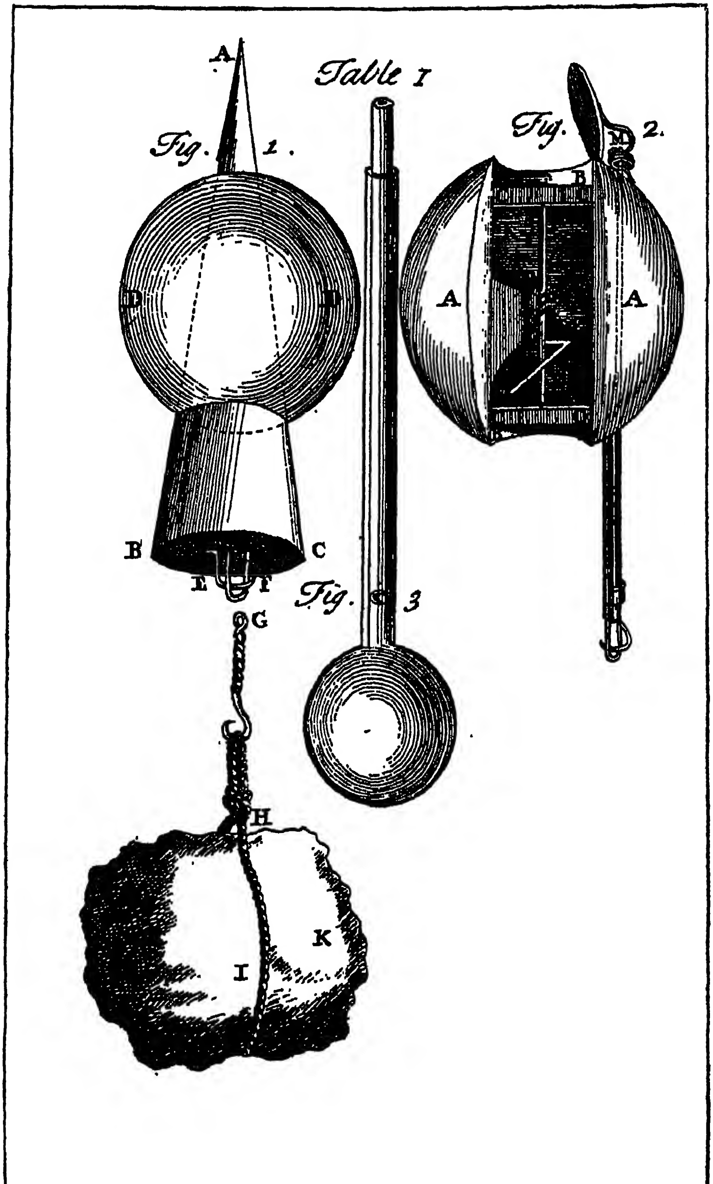
In the *Philos. Transact.* N. 9, and 24, we have a description of an instrument, to sound the greatest depths of the sea: but there were two great difficulties that attended it: the first was, that it was necessary to make the weight, that was to sink the ball, of a certain size and figure, so proportioned to the ball, as that the velocity of them, downwards, when united, should be equal to the velocity of the ball alone, when it ascended in its return; in order to which, it required to be prepared with care, and required also some charge, it being almost necessary to make it of lead, of a certain weight and figure. The other was, the difficulty of discovering the ball at the first moment of its return, which was likewise of absolute necessity; and it was likewise necessary to keep the time most exactly of its stay, or continuance, under the surface of the water, by the vibrations of a pendulum, held in one's hand; for I was inform'd, that, upon trial, they have, after some time, perhaps discover'd the ball floating in a place, where they did not at first expect it; and so that experiment became insignificant, tho' they were at the charge of losing the leaden weight, and had used all diligence to keep the time, and to watch for the first appearance of the ball.

This way, which I shall now explain, is freed from all these, save only of finding and recovering the ball, after it is returned from the bottom; for I have no need of proportioning my weight, provided it be heavy enough to sink, nor of making it of this, or that figure, or of lead, or any other metal, since a stone, if big enough, of any shape, will do; nor have I any need of counting the time of its being under water, since it will do as well, if I procure the ball an hour after it floats; so that all the trouble is, the fetching in the ball, when 'tis discovered, and the letting it into the water, when it begins to sink.

It remains therefore only to describe the means and way, how this matter is to be effected, and 'tis, in short, no other than what I then experimented, and gave an account of, in writing, to this Society; as, I believe, will appear by the Register of that time, which was, as well as I can remember, in the year 1661, or 1662; but because few here, now present, may remember it, I shall now again describe it.

It consists then of three parts; the first is a stone, of a sufficient bigness, to sink it to the bottom, how deep soever; and the bigger the stone be, the more expedition doth this messenger make to its stage. Secondly, of a wooden ball, well pitched, which is carried down, by the stone, to the bottom, which then leaving it, it returns, with speed, to the top, and there floats upon the water, from whence it is to be fetched aboard. Thirdly, of a cylinder, cone, or hyperbolick trumpet, that is to bring back the information to what depth it hath descended; this is fastened to the ball, in the manner described in the figure; and at the bottom of this is fastened the cock, or crook, by which they are both pulled down to the bottom, and then let loose in the same manner, as was practised on the former, described in the *Philosophical Transactions*.

The cylinder, hyperbolick trumpet, or cone, (*Tab.* I. *Fig.* 1.) A B C is to be hollow, made of tin, or thin brass, and so contrived, as, by a small hole, to receive the water into it, less or more, according to the external pressure at the apex A, of the fluid it descends in; so that it will always, by the quantity of water contain'd in it, give a true account of the pressure of the water, at the bottom, which is always proportionate to the depth of it, below the surface; this is shewn by the compression of the air included, whose dimensions are always in reciprocal proportion to the pressure. This is to be found after the ball is returned from the bottom, by weighing the quantity of water, contain'd in the cone, or other receiver, and comparing it with as much water, as will exactly fill it, or by a measure of capacity; or thirdly, if the receptacle be perfectly regular, by a gauging rod set in its axis; but the best, and most sure way, I take, to be by weight. D D is the ball, made of light wood, and well pitch'd, and of sufficient bigness, to raise up the cone, with its contained



Hooke's Instrument for Deep-sea Sounding.

water, as soon as it is discharged from the stone or weight, K K, which is to be of a weight sufficient to sink it, and then slip from it, at the bottom, by means of the spring-hook, E F. G the ring to be hung upon the hook F, H I the cord. There is nothing in the contrivance, but what is easy to be made, and the charge will not amount to a farthing a trial.

Emissarius secundus ad fundum Abyssi, sive Explorator Distantiæ Inanimatus.

The opinions, concerning the abyss, seem to have been received, and conveyed to us, from the first and most ancient times of the world. And we find that *Ovid*, tho' he seems to have understood the earth to be spherical, yet he, speaking of the creation, and first production of things, (of which no doubt, he received his information from the writings of *Moses*, or some other that had seen them) makes the water to be the lowest of all the elements ——*Circumfluus humor ultima possedit solidumque coercuit Orbem.* I had no further intention, but to shew, that the sea was call'd the abyss, and by the abyss was meant a depth, not possible to be founded, or measured, by the power of art: but it is more properly rendered, by our *English* translation of the Bible, *the deep, or the great deep*, (when the depth of the sea is meant) than by the abyss in the *Vulgar*; yet there are several expressions that do shew, it was understood to signify a depth, that was beyond the power of man to measure; and so it seems to be meant in the first chapter of *Ecclesiasticus*, where 'tis said, *Who hath measured the height of heaven, the breadth of the earth, or the deep*; that is, the profundity of the sea. And so the expression in the 37th chapter of *Job* seems to intimate: the expressions in the Scripture, relating to physical matters, being accommodated generally to the most common and receiv'd opinions of men, concerning them. Certain it is, that no one, yet, hath experimentally found what the greatest depth of it is, except only in such places as are measurable by lines and a plumbet, and that, for the most part, near some land. The greatest that I have met with, of that kind, which I can rely upon, is, what Mr. *John Greaves* relates, that he tried in the sea. The passage is in the 102d page of his *Pyramidographia*. *In the longitude of* 11 *degrees* (says he) *and in the latitude of* 41 *degrees, having borrowed*

the tackling of six ships, and, in a calm day, sounded, with a plumbet of almost 20 *pound weight, carefully steering the boat, and keeping the plumbet in a just perpendicular, at* 1045 English *fathoms; that is, at about an* English *mile and a quarter, in depth, I could find no land or bottom.* These are his words; but where this place was, I cannot define, because he does not declare from what meridian he computes his longitude: whether it were in the ocean, to the westward of *Portugal,* or in the *Streights,* about *Merseilles;* where-ever it were, it was an excellent place, to have tried many curious and instructive experiments, that might be there tried, by such as have opportunity to go that way again, if it were certainly known. But this depth is nothing, in comparison to what *Hesiod* supposes it, or *Tartarus,* which is the same abyss; namely, as deep downwards, as the heaven is high upwards; and that he asserts to be so high, that an iron axe would be 10 days in falling, before it would touch the earth; and just so long would the axe be falling, before it would arrive at *Tartarus.* But to leave these poetical fictions, certain it is, that the sea is, in some parts of it, very deep, and it would afford many useful informations, if inquiries were carefully made, by means of my *explorators,* or *nuntii inanimati*; for by some, or other of them, one might be ascertain'd of divers things, yet, never known to mankind. That which I described, the last day, was, for measuring the distance, which it would effectually do, were it not for one objection or two, which may possibly render its account disputable. The power of the spring of the air, is most certainly in reciprocal proportion to its dimensions, to whatever bulk the same air be reduced to, by compression; 'tis certain also, that the compression or trusion, of a fluid body, is always in proportion to the length of the cylinder compressing; so that the power of compressing of any one cylinder, of a certain height, being known, the power of any other cylinder, whose length is given, is easily found. The like is to be estimated concerning the powers of the resistance of the air, if its power of expansion, or resistance to condensation, be known, for any one expansion, or dimension of it; the power of expansion is known, for any other dimension of it given. So that both the principles, upon which the last day's experiment was founded, are undoubtedly true and genuine, and consequently

the invention, thereupon founded, would succeed; and then, the first *nuntius inanimatus* would be a true *explorator distantiæ*. But there are two things that may make its information dubious; the first is, the uncertainty of the temper, as to heat and cold, in those very deep, sub-marine regions. For we know that heat does augment the power of expansion in the air, and cold doth diminish it; and therefore it will be uncertain, whether all the contrusion of the air, at the bottom, be to be ascribed to the gravity of the incumbent cylinder of water, or to the coldness of the water of those regions, in part; till therefore the temperature of those regions be known, we cannot positively affirm, what part of its condensation was to be ascribed to the incumbent cylindrick height of the water.

Explorator Temperamenti.

To know this, I have another messenger, call'd *explorator temperamenti*, which shall fetch a true account thereof; and so that objection, or impediment, would be easily enough removed, if need were.

But there is another objection (which is also very material) against the aforesaid method, and that is this, that, as 'tis true, that if the water, at the top and the bottom of the sea, were all of a uniform nature, then the rule for its gravitation, or pressing, would hold exactly according to the rules of proportion, I have before premised, and the deductions therefrom would be indisputable; but if there be differing sorts of water, in differing depths, as no one has yet ascertain'd us of the contrary, then differing sorts of water will give differing degrees of gravitation, or pressure; and the proportion I have assign'd, for an uniform cylinder of water, will no longer be of use; for if the water in *specie* be heavier, (as most probably it is) then a shorter cylinder of it will have the same power to press, that a longer cylinder hath, of a water lighter in *specie*; so that if the water be twice as heavy, half the height will produce the same effect; and if thrice the height, then a third part of the cylindrick height will be only necessary; and if it should be as heavy as quicksilver, or indeed as heavy as the stone, or weight, that sinks the ball; then the *explorator* will not dive into it at all, but stay at the top of it. It is necessary therefore, that we be ascertain'd of the nature and condition of the

water, or liquor, whatever it be, at the bottom, or in those lower regions, at any assignable depths.

Explorator Substantiæ.

And for these purposes I have other *explorators*, that shall bring me a certain account, what kind of water, or other liquor, it is that possesses such, or such a depth, be it 500, or 1000, or 1500, or 2000 fathom deep, or any other greater, or less, assignable depth; these I call *exploratores substantiæ*, and of these I have several kinds, according to their several employments and business. There is yet another scruple that must be removed also, and that is, whether the gravitation, towards the center of the earth, do continue the same, at any depth; or whether it do increase or diminish, according as the body is posited lower and lower, beneath the surface of the sea; for if gravity do increase, then the body will move downwards, or sink faster, than at the top; and if it decrease, it will do the contrary. Now there have been many, and, among the rest, the incomparable *Verulam*, that have affirmed, that stones, *&c.* in the bottoms of deep mines, do weigh much lighter, than at the top; if so, why may not that be true also, of the depths in the sea: however it be, it is desirable, in philosophy, to be ascertained, whether it be so or not; and if it be so, what the differences really are; for which purposes I have other *nuntii* or *explorators*, that shall certainly inform me, concerning those particulars also. There are many other particular inquiries, which one would desire to be ascertain'd of, which I shall afterwards mention, and also furnish or supply messengers, sufficiently accoutred, to bring back informations, certain and instructive. But I shall not trouble you with them at present; but if there be an opportunity of trying these I have named, and many other I could enumerate, I shall be ready to give my assistance: they are experiments indeed, not to be tried in the presence, or at the meeting of this Society, but yet they are such, as it were, very desirable, that the Society had a true account of them; as there are also thousands of others, which, it were to be wished, this Society would procure informations of; which, I conceive, is in their power to effect, if due means and methods were made use of, for effecting those ends. The harvest is great, but the labourers are few; and without hands and heads too,

little can be expected; and to rely only upon time and chance, is, probably the most likely way to have all our hopes frustrated.

Explorator Profunditatis.

But to leave this digression, I shall, at present, only describe another messenger, who is to be *explorator profunditatis*, or a true surveyor of the distance, which it is not at all liable to the uncertainties of the last, or any other, as I conceive; for be the heat or cold, of that climate, what it will, or whatever the density or rarity, whatever the gravity or levity of the water, whatever the gravitating power, whether the same, greater, or less, whatever the spring of the air be, *&c.* none of these, or any other, that I can think of, will be material, but the messenger will return, with a true account of what he was sent to inquire.

This *explorator* has divers parts, much the same with the former; as first, a large ball of wood, or (*Tab.* I. *Fig.* 2.) some other convenient material, which may be able to rise from the bottom, after the weight, that sunk it, is separated from it; this ball is marked in the figure A A; this has a cylindrick hole, B B B B, open quite through the middle of it, that the water may pass freely thro' it, as it descends to the bottom; in this I place two plates, C C, C C, edge-wise, to the passage of the water, which have each a center-hole to receive; and hold the pivots of an axis F F, so as to move freely therein: upon this axis are fastened 4 vanes, in the manner as I have formerly describ'd, for measuring the way of a ship thro' the sea; these are marked with E E E; this axis has a screw pinion on it at G, which every revolution turneth one tooth of a wheel of account, H, whose pinion turneth I, whose pinion turneth K, *&c.* these keep a certain account, how many revolutions the vanes do make, in their passage to the bottom; and these revolutions do measure the body of water, they have passed thro', in their whole descent; but that the rising of the ball may not cause the vanes to return backwards, I have several inventions; that I shall mention, at present, is very easy, namely, a lid, or cover to the cylindrick passage, which is shut so soon as ever the weight leaves the ball, which I effect by the spring M, which is kept down close to the ball, whilst it is descending, but springs up so soon as the weight is left, and by that means it shutteth the cover N, which stops the cylindrick hole.

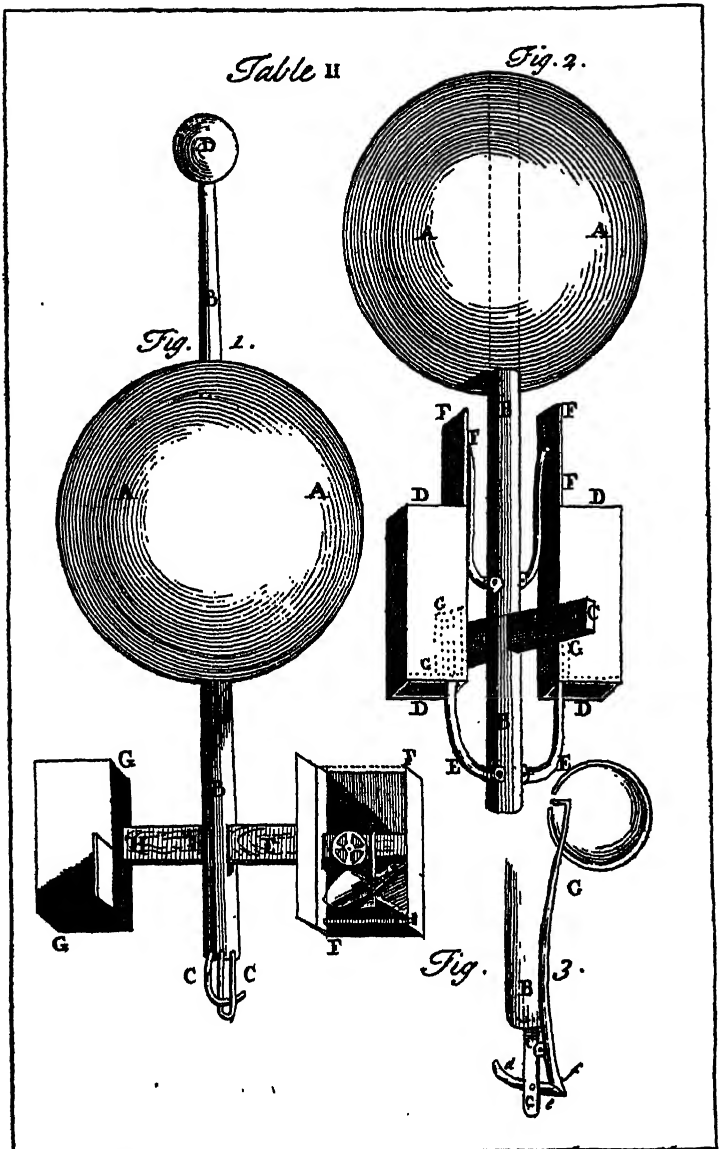
Dr. Hook's Lecture, read Dec. 16, 1691.

In my preceding lectures, I have described two of my *nuntii inanimati*, or *exploratores abyssi*, whose business it is to bring back a certain account of the distance, or space, between the top and the bottom of the sea, which I made choice of, in the first place, to equip, they being previous, and the forerunners of all the rest. The first of these, tho' it would do well enough in moderate depths, where there is no great difference in the temperature of the water, as to heat and cold, and other qualities; yet in greater, especially in very profound depths, I conceive, it may be liable to uncertainty, for the causes I did the last day mention; which to prevent, and obviate any other cause of doubt, which I could, or can yet think of, I did contrive the second *explorator distantiæ*, which I described the last day; the contrivance of which is such, as, I conceive, will most exactly measure the said distance, and bring back the true account thereof. The way I mention'd, the last day, was contrived only to measure the length of its descent; which, I conceive, will be sufficient assurance of the extent, or depth, thereof. However, if any shall desire to be more ascertain'd of the truth and exactness thereof, I have contrived a variation of, or addition to, the same, which is only another prismatick box, or hole, with the same kind of helical vanes and wheels of account, as the former had, which is so adapted to the float, and contrived, that, all the while the weight is descending, this additional way-wiser shall stand still; and so soon as ever the float is freed from the weight that sank it, and it begins to ascend, this doth then begin to move, and so continues, till it arrive at the top of the water: so that as the former did measure the length descended, so this doth measure the same ascended; which if they be found to agree, 'twill be a double confirmation of the certainty of the experiment. I know it will be objected, that this will make the *apparatus* very chargeable and difficult; and (as seeming complicate) to be apt to be out of order; and few will use the caution and circumspection, that such an instrument will necessarily require: to which I answer, that I can make the whole so easy, and obvious, that the whole instrument need not cost above a crown; and that any one, almost, shall be able to make, or to

mend it; and any one, that can but write and read, can be able to make trial therewith, and keep account thereof; nor will it easily be so out of order, but that it may easily be mended, and set to rights again. This, I conceive, will do; all that needs to be done, to perfect this enquiry, which being the first, and principal, I have been the more curious, to obviate all objections, and to reduce it to as easy and plain a way, as can well be desired, considering the many difficulties which are to be provided against. I have not made a module of this third, and most compleat contrivance of all the three; but I have prepared a draught, so that those, who understood, and remember the contrivances of the first and second, will easily comprehend the fabrick of this.

The third Explorator Distantiæ.

A A represents the ball, or float of wood, through which is put B B, a stick smaller, and tapering upwards towards great D, which is an hollow, very light ball of wood fixed on the top of it, for the more notable sign, or signal, (by which to find it, in the sea, after its return) but bigger, and more substantial downwards, that it may be the more fit to hold the staple, and hook at the bottom C C, and likewise the crosspiece E E, which passes through a mortice made in it, and is thereby kept at right-angles with it; upon the ends of this cross-piece, E E are fixed two prismatick boxes, F F, and G G; F F is the box that holds the vanes and way-wiser, made after the same manner, as was that of the second module, which I shew'd the last day, with no other difference, but that in this contrivance, the box is shut by the water, so soon as ever it begins to ascend, without any need of the spring which I had made in the second; and that the box is made to open one side, the better to fix the vane and way-wiser; and likewise the inside of it is square, the better to be kept steady in the water, so that it shall not be winded, or twisted by the helical vanes; which it would be more apt to be a little, if the hollow of it were truly cylindrick. (*Table* II. *Fig.* 1.) G G is exactly the same kind of prismatick box, with vanes, and way-wiser, as the former, but it is perfectly inverted, with respect to the former; for in the former, the valve, or lid to cover it, is placed, or fixed by hinges, to the top, so that the water shuts it, and keeps it so, all the while it ascends. In this, G G the valve, or lid, is placed at the bottom,



Hooke's Instrument for Deep-sea Sounding.

and remains shut all the way it descends; but so soon as it begins to ascend, 'tis opened, and the vanes are turned by the boxes passing through the water. The contrivance, for the opening and shutting these lids, is by an equal flat, fixed on the axis of each, at right-angles with them, that of the ascending way-wiser, G G, is drawn, and marked with H H in the figure. Thus, I conceive, I have sufficiently accoutred my first *explorator*, who is to inform me of the depth; and is, indeed, to be the general post that must fetch me all the other informations I desire.

The Thermometer, or Explorator Temperaturæ.

In the next place, I desire to be informed of the temperature of those lower regions, as to heat and cold: and for this purpose I have contriv'd a thermometer, that shall certainly inform me; this is nothing but a small bolt-head, filled up with spirit of wine, to a convenient height of the stem, with a small embolus and valve; the embolus is made so, as to be thrust down the neck, as the spirit of wine shall be contracted by cold; and the valve is to let out the spirit of wine, when it is again expanded with heat, in its ascent; 'tis very plain, and easy to be apprehended, especially when that is viewed, which I have here provided: it may, possibly, be thought that the great pressure, of the incumbent body of water, may contribute somewhat to the contraction, or shrinking, of the spirit; but tho' I am inclin'd to think, that that will not cause any sensible variation, yet, to try that, I shall shew a means how it may be discovered; which discovery, of it self, will be a discovery very considerable, (*Tab.* I. *Fig.* 3.) because none of the ways, that have hitherto been attempted, have proved effectual, for the condensation of any fluid, by pressure only, though there have been made many experiments, by this Society, on purpose for such a discovery.

Explorator Gravitationis.

Next I desire to be informed, whether the pressure of the water do exactly keep the proportion which I have assign'd it: and for this purpose, the perforated cone, described in the first *explorator*, sent down, and brought back with the *thermometer*, will give an account thereof; for by the *thermometer*, (*Table* I. *Fig.* 3.) we shall be informed, what is the degree of cold, and consequently

we shall know, what part of the condensation of the air, in the cone, is due to that, and consequently what part is to be ascribed to the pressure; and by the way-wiser, or third *explorator*, we are assured of the depth, and consequently we may know, whether those do answer to each other, according to the theory, or proportion assigned.

This I mention, to show that no one of the instruments, I have already described, or shall, for the future, explain, are useless, or superabundant; for that, before I leave this subject, I shall shew for what peculiar use each of them is principally designed, tho' many of them will not serve for that one, but for the assistance of many others; where they will be of as necessary a use, in concomitance with others, as they are singly necessary for that end, for which they were principally designed.

It may possibly be queried, why I make use of spirit of wine to fill my thermometer, and not of water, or other liquor: to which I answer, that first I found by many trials, which I purposely made, to perfect that kind of thermometer, (of which, I believe, I made the first that were made in *England*, from the sight of a very small one, brought out of *Italy*, about 30 years since, by the President) that this spirit was the most sensible of any liquor, I could then meet with, of the degrees of heat and cold. And secondly, because this liquor was capable of enduring the greatest degree of cold, I could give it, by the means of salt and ice, and yet remain'd fluid, without congelation, but did continue to shrink to the last. Now what the temper of the sea may be, at those vast depths, whither this is design'd to be sent, no man now living, or that ever did live upon the earth, hath experimentally known, (as I am, with good reason, persuaded). But, by conjectures, one may be induced to expect, that the cold should be there very predominant, and, in probability, such as would congeal, and turn to ice, a body of fresh water. And 'tis, in probability, one of the causes that the sea was made to abound with salts, by the Divine Providence, who adapted every thing to its proper use and end; for 'tis very hard to suppose, that the heat of the sun should communicate so powerful an influence from the top, or surface of the sea, downwards; for the parts of any uniform fluid, that are warmer than the rest, are also lighter,

and consequently will ascend upwards; but that the heated particles, at the top, should sink, or descend, 'tis not to be supposed. Again, that the light, and, possibly, somewhat of the heat of the sun, may be communicated to the bottom, if the water be clear, 'tis not to be denied, but then it must be so small a part, of what we see necessary, to keep fresh water from freezing here above; first, by reason of the quantity reflected by the superficies of the water; and secondly, by the opacities, that must necessarily obstruct their passage, thro' so vast a thickness, that no part, near the poles of the earth, can receive so little benefit of these two qualifications of the sun, as these parts must needs do. It seems therefore reasonable to me to suppose, that where there is such a defect of heat, nature does supply a more copious quantity of salt, or some other such body, as is able to resist congelation, whether saline or metallick; as quicksilver, or such like, time and experiments may inform us: which experiments, how they may be made, I shall, the next day, inform you, and furnish you with such emissaries, as shall bring back a true account of what kind of substance the mass of the sea is composed, at any assignable depth, not only at the bottom, but of any interjacent part assigned, between the top and bottom.

Lecture read Dec. 23, 1691.

I have in my preceding lectures endeavoured to shew by what methods, and by what kind of instruments, we may be experimentally ascertain'd of several desirable informations, about the lower regions of the abyss, or great deep. As first, and principally, what the depth of the sea may be, in any place we desire to measure it; and this by several instruments of differing construction, and upon different principles; the last of which, I conceive, to be so compleat, and perfect, as to obviate any objection that can be made against it; as particularly that which was objected the last day, that if the water should move upwards or downwards, (tho' such kind of motions cannot, with any ground, or probability, be imagin'd, or supposed, since the bottom, or ground, is a bound to the water below, and the superficies, or air, is a bound to the water above; so that unless there be a vent one way, that is downwards into, or out of the earth, or upwards, into the air, there can be no reason given why there should be

such a motion) but it may be said, that there may be, in some places, some such *Voragoes*, as Father *Kircher* imagines, in his *Mundus Subterraneus*; that is, such subterraneous passages, as convey the water of the sea from one place to another: of which kind he tells us of many, tho', I doubt, it will be difficult to prove any one of them. I know, indeed, that Mr. *Hacluit* hath taken a passage out of *Gerrardus Mercator's* General Map, which doth hint at some such extravagancies; his words are these:

'Touching the description of the North parts, I have taken the same out of the voyage of *James Crogen*, of *Hartzeron Buske*, which alledgeth certain conquests, of *Arthur*, King of *Britain*; and the most part, and chiefest things among the rest, he learned from a certain priest, in the King of *Norway's* Court, in the year 1364. This priest was descended from them, which King *Arthur* had sent to inhabit these islands; and he reported, that in the year 1360, a certain *English* Friar, a *Franciscan*, and a mathematician of *Oxford* (possibly he meant *Roger Bacon*, or some of his disciples) who leaving them, and passing further, by his magical art, described all those places that he saw, and took the height of them with his astrolobe, according to the form that I (*Gerrard Mercator*) have set down in my map, and as I have taken it out of the account of the aforesaid *James Crogen*. He said, that those four indraughts were drawn into an inward gulf, or whirlpool, with so great a force, that the ships, which once entered therein, could, by no means, be driven back again, and that there is never so much wind, in those parts, as to drive a corn-mill.'

Geraldus Cambrensis (who flourished in the year 1210 under King *John*) in his book of the Miracles of *Ireland*, hath certain words, altogether alike with these; *viz.* 'Not far from these islands (namely the *Hebrides*, *&c.*) towards the north, there is a certain wonderful whirlpool of the sea, whereunto all the waves of the sea, from far, have their course and recourse, as it were, without a stop; which (these conveying them into the secret receptacles of nature) are swallowed up, as it were, into a bottomless pit; and if it chance that any ship do pass this way, it is pushed, and drawn with such violence of the waves, that eftsoones, without remedy, the force of the whirlpool devoureth the same.

'The philosophers describe four in-draughts of this Ocean Sea,

in four opposite quarters of the world; from whence many do conjecture, that as well the flowing of the sea, as the blasts of the wind, have their first original.' Thus far is Mr. *Hacluit*'s quotation of *Mercator*. Mr. *Hacluit* adds, in the margin [*There is a notable whirlpool on the coast of* Norway, *call'd* Malestrome, *about the latitude* 68.] The best account of this *Malestrome*, that I can learn, is, that it is a circulation of the water of the sea, caused by some submarine rocks. But Father *Kircher*, who is good at fiction, has found a subterraneous passage for it, into the end of the *Bothnick Gulf*, and from thence another, into the *White Sea*, not far from *Archangel*. I grant such a passage may be possible, but I should be glad to have it proved; or indeed, any one of those many, which *Kircher* has asserted, in his *Mundus Subterraneus*. So that if there be any such place in the world, it is not yet found out, or proved: and therefore there is no great cause of supposing many, or making that an objection against my third *explorator*, who will perform his business, tho' that were actually so; that is, tho' the motion of the water were directly upwards, or directly downwards; and not only that, but it will also, over and above, tell you, what that motion is. This is evident, by comparing the ascent with the descent, for half the sum will be the true depth, and half the difference will be the motion of the water, whether upwards or downwards, which the way-wisers will certainly inform you of. But this, I suppose will be needless; however, I was willing to remove the stumbling-block, tho' it was but a straw.

Explorator Qualitatum.

To proceed then, I shall next shew how to fetch a quantity of water from the bottom, or from any intermediate space, or distance from the top.

This I perform, by means of a bucket, the same I have formerly here describ'd, and verified by trials; or by another contrivance not much unlike it, which I shall by and by describe. The former contrivance will serve indifferently, both for fetching the water from the bottom, or from any intermediate part; but for the intermediate parts, there is an additional contrivance, or invention, for freeing the float from the descending weight, or stone, after it hath been carried down a certain number of

fathoms, which the following plain contrivance will effectually perform, at any determined distance, let it be 100, 500, 1000, 1200, 1500, 2000, or more fathoms required, where there is first found to be depth, sufficient for to make such experiments, which is necessary to be first well assured of by the third *explorator distantiæ*; because, if the depth be not sufficient, that is, if the stone, or descending weight, do touch the bottom, before it hath descended the designed number of fathoms, it will detain the float, and not dismiss the *explorator*, to return with its message. The reason of which, you will presently apprehend, when I have describ'd the invention for the performance thereof; tho' yet, with a small additional spring, it will serve for both purposes. I make use of the third and last *explorator* for this purpose. I fit to it two buckets of wood, made, according to the contrivance I have formerly describ'd;[1] these are fasten'd to the lower end of the stick, which passeth thro' the ball, or float, as I shew'd the last meeting, and the buckets are set at right-angles, to the bar that carries the way-wisers, or mensurators, as appears in the figure which I have here design'd, where A A represents the ball, or float; B B the stick thrust thro' it; C C C C the cross-bar, for carrying the way-wisers; D D. D D, the two buckets, plac'd or fix'd by their arms E E. E E, to the said stick, at right-angles to the bar; C C. C C. F F. F F represent the covers at the top of each; and G G. G G, the valves, or shutters for the bottom; (*Tab.* II. *Fig.* 2.) These being within the box, or bucket, cannot be well expressed by delineation, but are faintly design'd by prick'd lines; and the description and modules, I formerly made, do make the design sufficiently plain. These valves, or shutters do stand open and upright, all the time that the float descends, and the water passeth freely through them, changing every bucket's length that the *explorator* descends; but so soon as ever it begins to re-ascend, they are presently closed, and shut into them their whole capacity, fill'd with the water in which they then are. This being then understood, for fetching up the water at the bottom, how deep soever, there needs no other contrivance than what I formerly describ'd; for so soon as the weight doth touch

[1] See the description of these buckets in *Philosophical Transact.* No. 9 and 24.

the bottom, the float, and all its furniture, is freed from it, and so is at liberty to re-ascend, and carry back with it, what it was design'd to fetch. But for fetching up the water from any intermediate depth, (as at 100, 200, 500, 1000, 1500, *&c.* fathoms below the surface) I have invented an easy expedient, which is to let go the weight, that sinks the *explorator*, at any station of depth design'd. I have already explained the *way-wiser*, or *mensurator* of the depth descended; one of the wheels of which doth keep account of every hundred fathom descended: upon this wheel I put on a springing round plate, with a hoop about the edge of it, which hath one notch in the circumference, or hoop; this notch I can set against any number of the plate, in the same nature as 'tis common for setting the alarm of a clock, to go off at a certain time designed; which, to effect, I have contrived a very easy expedient, which the third figure doth represent. (*Table* II. *Fig.* 3.) Suppose then B B, to represent the lower end of the stick that hath the way-wisers and buckets, in the end of which is fixed C *c*, which is a staple made of a flat iron plate; between the sides of this is fastened, by a pin *c*, the hook *d e* by the end *d* of which, doth hang the wire of the weight; this hook is kept in this posture, by a small piece of wood or iron *f g*; the end *f* is cut sloping, to answer the slope of the end *e*, of the hook *d*. Now so long as the end *g*, of the trigger (as I may call it) is detain'd within the hoop of the wheel of account *h h*, of the way-wiser, so long is the hook, *d e*, kept firm in the posture it is here designed, and so retains, or holds the float and furniture fast to the descending weight; but so soon as the way-wiser has measured the number of fathoms designed, and the notch in the hoop be brought to the place, where the end of the trigger *g* may slip out, the hook has no longer any power to hold fast to the descending weight, but presently lets it go, and the float returns, and the buckets close, and bring back their bellies full of the water of that part; or the temperature, if the *thermometer* be hanged to the stick; or the pressure, if the cone, together also with the degree of gravitation.

I shall only add one more enquiry to be resolved of at present, and that is to know, what alteration so great a condensation, or compression, as must necessarily be caused at so great a depth,

will be produced in the body of the air, so condensed; that is, since the air is but about 7 or 800 times, at most, lighter than water, and that 2200 fathoms pressure will, according to our theory, reduce it to as dense a body; whether, I say, this condensation will not actually reduce the whole body of the air, so condensed, into perfect water. This may be easily tried, by letting down, with the *explorator*, a small glass bolt-head, filled with air, with the mouth of the stem, or neck, turned downwards, and contracting the end of the same, by a lamp, into a small perforation, to let in the water thereby under the air, as it descends, and to let out the water, if the air do again expand, as it re-ascends. This is so easy to be apprehended, that I thought it needless to add any delineation, for the further explication thereof.

1691/2

February, 1691–2.

Dr. Hook's *discourse concerning Telescopes and Microscopes; with a short account of their Inventors, read in February* 1691–2.

[Derham, p. 257.]

Of Friar Bacon, Baptista Porta, Diggs, Metius, Galileo, *and other Inventors of* Telescopes.

How much the greater improvements of natural knowledge have been owing to the discoveries and improvements that have been made in opticks, I think few can be ignorant of, that have inquired into the reasons and grounds of the progresses made in this last century, since it hath been actually effected: for, though it be evident that *Roger Bacon* did understand somewhat of the grounds of it, and, in probability, would have further improv'd that his knowledge, if he had met with a generation worthy thereof; yet such was the ill treatment he receiv'd by false accusations, scandalous reports, imprisonment, and loss of places, that we hear no more concerning it, but only some hints that he gave, of his being able to see things at a distance as if they were near, in his apology for himself, addressed to the then Pope, to protect him against his persecutors. This persecution quash'd it for that time; and we find nothing of the revival thereof, till the *Lyncean* Academy became founded in *Italy*; where, from the encouragement that divers ingenious men

received, it was again started: and we find that *Johannes Baptista Porta* had made a discovery of it, as is very plain by some passages of his natural magick; and our *Diggs* had done the same thing here, as is testified by his son, who printed some of his father's works after his death. These two testimonies we have, that somewhat like the telescope was known in the preceding century, both the said books being printed before the beginning of this century. We find nothing further concerning its description, or use, besides the hint that it was then known to these two men, some years before *Galileo* put it in practice. In the beginning of the present 17th century, *Metius*, a spectacle-maker in *Holland*, light upon a composition of a convex, with a concave glass set at due distance in a tube, which made a perspective glass to see objects at a distance. And *Galileo*, in *Italy*, whether excited by a hint thence received, or from *Baptista Porta*, or by his own good genius, is uncertain, did the same thing at *Florence*: but not contented with the bare invention, and use for terrestrial objects, he improved it farther, and made use thereof for discoveries of the cœlestial bodies. By this means he detected the *Galaxina* to be an infinite *Congeries* of small stars; as also the cloudy stars, to be of a like composition. By the same he discovered the roughness and inequality of the surface of the *Moon*, and the phænomena of the shadows and lights of those rough and uneven parts, and the progress and recess of the light of the *Sun* thereupon. By this he discovered the four stars about *Jupiter*, and in some sort adjusted their periods, and hinted the use of them, for the discovery of the longitude of places upon the earth. By this also he discover'd the unusual figure of the body of *Saturn*, the waxing and waining of the light of *Venus*, and the spots in the face of the *Sun*, together with their motions and changes; which last, whether it were not primarily, or at least at the same time, detected by *Scheiner*, is disputable, since both lay claim to it. This, I think, may truly be said for *Scheiner*, that whoever first detected them, he was the man that perfected the theory of them, so far as it has hitherto gone; which he hath performed in that most elaborate work of his *Rosa Ursina*.

These discourses excited the curious of those times to inquire into and improve the knowledge of opticks, especially that part

of it which had been least cultivated, namely, the business of refractions. (*Stelliola*, who was a *Lyncean*, seems to have been the first that discover'd the ground of refraction, in his book *Il Telescopio overo il Specillo Celeste*.) *Kepler*, in his opticks, explain'd the reason of the phænomena of *senses*, and the causes thereof; and also, that the spherical surface did not give the true figure requisite to refract all the parallel rays that fell upon it to one point, but a figure somewhat elliptical; but made no demonstration what the true figure was, nor the true proportion of refraction. But *Descartes*, by these two helps, went through with the demonstration, and proved both the true elliptical figure, and also most ingeniously and mechanically explain'd the ground and cause of refraction.

Fermat soon after, taking a contrary supposition, explain'd the same phænomena; as did also *Emanuel Maignan*, in his *Perspectiva Horaria*, by a third supposition; and our countryman Mr. *Hobbs* by a fourth; but these two last by ways less intelligible and more improbable. Others since have gone other ways, but fall short of the first. However, the first successes caused it to be exceedingly cultivated by very many ingenious men. And that not only as to the theory, but as to the practice also: thence many attempts have been made by divers ingenious men, as *Descartes*, *Hevelius*, Sir *Paul Neile*, *Divini*, Mr. *Smethwick*, and others, to make object-glasses and eye-glasses of elliptical figures, but all without success. However, of the spherical figure they made good improvements, by making object-glasses of much greater lengths, and truer figures, than they were at first able to do: for, *Galileo's* glass, of which he made so good use, I have been informed, was not above four or five foot long, at the most; and, I am apt to think, that the glass, *Hevelius* used for his *Selenography*, was not better, if, at most, it were so good; since as many particulars, as he has noted in that book, may be made with a glass of three foot. But Sir *Paul Neile* made some of 36 foot pretty good, and one of 50, as I have been informed, but not answerable. *Divini* and *Campani* made also glasses of those lengths, but how good I cannot knowingly affirm: however, if we may be allowed to judge of them by the discoveries they made with them of the true figure of *Saturn*, I conceive they were but ordinary,

and did not exceed our 12 or 15 foot telescopes; for, by one of that length, I plainly discover'd the ring and satellite of *Saturn*, to be as Monsieur *Hugenius* doth assert in his book; and, with the same telescope, I first discovered the permanent spot in the belt of *Jupiter*, which proved its diurnal motion on its axis. Since that, Mr. *Reive* first, and then Mr. *Cox*, made some good glasses of 50 and 60 foot long, and the last one of 100; but how good, I cannot assert, having not made trial of it. And, as it hath been cultivated here, so others, in *France* and *Italy*, have not been idle: particularly one Mr. *Borelli*, at *Paris*, who presented one of a considerable length, to this Society, which Mr. *Flamstead*, I suppose, has in his keeping, Sir *Jon. Moor* having borrowed it of the Society for his use. But tho' there has been some life left in the grinders of glasses, yet the warmth of those, that should have used them, has grown cool; and little of new discoveries hath been made by them, besides what Mr. *Cassini* has done at *Paris*, in discovering four new satellites about *Saturn*, besides that of Mr. *Zulichem*.

Much the same has been the fate of microscopes, as to their invention, improvements, use, neglect and slighting, which are now reduced almost to a single votary, which is Mr. *Leeuwenhoek*; besides whom, I hear of none that make any other use of that instrument, but for diversion and pastime, and that by reason it is become a portable instrument, and easy to be carried in one's pocket.

If we enquire into the reason of this change of humour, in men of learning, in so short a time, we shall find that most of those, who formerly promoted these enquiries, are gone off the stage; and with the present generation of men the opinion prevails, that the subjects to be enquired into are exhausted, and no more is to be done: besides, they pretend that all the discoveries that have been hitherto, or that can be made, for the future, by these instruments will afford no gainful profit, and all other notions are insipid with them, besides such as bring ready money.

But those, who make such estimates, may, perhaps, find themselves very much mistaken in their judgment, if the subjects were duly prosecuted, as they are capable of so being. For, as to the discoveries that may be made in both kinds, I conceive

they are vastly greater, both for number and value, than those few that have been already made; and not only for the information of the intellect, but what answers their greatest objection, even for the increasing their treasure.

Having given this short account of the history of telescopes, as also of the use and discoveries that have been hitherto made with them, which, as they have been very considerable, as to the improvement of the physical or natural knowledge of the cœlestial phænomena, I may observe that a further improvement and use of them, will, in all probability, afford much greater, and more considerable, not only for the perfecting and compleating the knowledge of those particulars which have been already, in part, detected; but also for making of other new discoveries, which as they are yet much further removed from the power of the senses to comprehend, so they have been, upon that account, never afforded entrance into the imagination and intellect; if at least *Aristotle's* maxim be true, that there is nothing in the intellect, but what was first in the sense: and tho' there are many things that may be imagined, and guessed at, by analogy, and the uniformity of the proceedings and productions of nature; yet there are certain non-pareils of nature, of which kind, possibly, nothing like them have been produced in all those particulars, which are more common and obvious, as I might instance in the body of *Saturn*. For who would ever have imagined such a configuration or fabrick, as that of the ring of *Saturn*? what is there in all the other celestial bodies, we yet know, that is analogous to it? and from the imperfection of the first telescopes, what extravagant and irrational conceptions were formed thereof, as does more evidently appear, by the descriptions and explications of the phænomena of it, before the more perfect discovery made by Mons. *Chr. Huygens*, and his ingenious explications thereupon. And that *Autopsia* is not only useful, but absolutely necessary, to give one a true *Idea* and conception of many phænomena, without which, the imagination is very apt to rove, and go out of the true way, as I might confirm by many instances, there being enough; but I shall only mention one, namely, that of Dr. *Vossius*, his explication of the phænomena of the moon, published in his last book, upon which I did formerly read a lecture to this Society,

to shew the irrationality thereof, and how little ground or probability there was to be found in all the phænomena of that planet, viewed and examined with a good telescope. And therefore I did conclude, that the learned man did never, himself, observe the phænomena, or if he ever did, it was certainly with a very small, and very imperfect, telescope. Upon which account, *Autopsia* is not only necessary for directing the mind and intellect, in its progress to be made, for what is to be gone thro' with; but 'tis necessary also, for the reducing it to its right way, from which it may have been misguided, by the false and erroneous suggestions it hath formerly met with, either in some famous authors that have positively asserted, or defended a falsity; or of some other person reputed eminently skilful in this, or that part of knowledge. With which kind of information, how full are the authors that have treated of some subjects? and that not one or two, but hundreds, nay, thousands, if we consider natural philosophy and physick, with the arts subservient thereunto: what shall we say to the whole generation of astrologers, which have yet always prevailed, and possibly always will, with some especially, who have once been prepossessed or prejudiced for it: the like may be said of those who defend the four *Aristotelian Elements*, or the four *Chymical Principles*, or the three *Cartesian Materia's*, or his *Mundane Vortices*, which are, in probability, all alike *Chimera*'s which have sprung up, and got rooting in the minds of men, in several ages of the world; and having once prevailed, they become prolifick, and propagate themselves in new soils, and new assertors and defenders of those doctrines do daily spring up: among these may also be ranged the *Solid Orb Men*, the *Plastick Faculty Men*, and the *Sympathy* and *Antipathy Men*, each of which, having once embraced their respective doctrines, will maintain and defend them to the last, against all others whatsoever. 'Twas from the first of these sects (as I may call them, from their division from the true philosophy) namely, the *Solid Orb Men*, that poor *Galileo* was put into the inquisition, and, to save his life, was necessitated to lose his doctrine, and to unsay what he really knew, and had discovered and asserted; and tho' he, as well as *Copernicus*, was encouraged, at the first, by Popes, Cardinals, and Princes, yet in the conclusion all fail'd, and their

doctrine must be condemn'd. Thus it happen'd also to *Roger Bacon*, and, I am apt to suspect, to the far greater man, the Lord Chancellor *Bacon*, for being too prying into the then receiv'd philosophy: But notwithstanding all this, there is a real beauty and allurement in truth, that will produce some votaries in the worst of times; and that will in time prevail, and shine out, and dispel the clouds of error that encompass it. *Multi transibunt & augebitur Scientia*, was the prophetick saying of *Daniel*, and used by the learned *Verulam*. And there is no doubt, but there is yet behind, much more to be discovered, than what is already known, if fit methods, and fit instruments be apply'd, and prosecuted with diligence. Some uses I have made of the *telescope*, and not without some considerable success; as in the discovery of the figure, motions and qualities of the *Cometical* bodies; as namely, of following them for near a month after they disappeared, and finding them retrograde, in observing their flame-like figures and qualifications; in discovering the smallness, or rather inconsiderableness of their parallax, by a way not taken notice of before, by any that I know of: and tho' Mons. *Cassini* has described it in his observation of the comet in 1680, yet he hath added nothing more to it, than what I published in my *Cometa* some years before, save the application of it to that comet. By these I discovered the parallax of the earth's orb, and the visibility of the fix'd stars, at all times of the day. Upon which occasion I cannot but take notice of a passage printed page the 385th of *Ozanam's* Mathematick Dictionary, and, by him, said to be written by Mons. *Cassini*; the sense is this; by the means of great telescopes, fixed to certain parts of the heavens, thro' which the fix'd stars pass, which are the most proper for this observation, one may best examine whether there be any difference (of the situation of those stars, as to *parallax*) in different seasons of the year; for this design, in the foundation of the Royal Observatory, there is left an opening thro' all the vaults, by means whereof one may see, from the bottom of the vaults, the vertical stars, thro' telescope glasses of 160 foot in length, which will be prepared against the Observatory is finished. Notwithstanding the *English* astronomers have begun to practise a method like to this, we are assured, by an essay of observations

which they have made with great subtilty, that they have found some such difference, which have verified that the diameter of the annual orb of the earth hath some sensible proportion, compared to the distance of the fix'd stars; which, nevertheless, is not yet evident to us, by reason that the observations, we have made of some fix'd stars variations, do not agree with this hypothesis; for that the variation was not found in the way that this hypothesis requires: but if the observations should confirm it, and be correspondent to the hypothesis, yet then we may doubt, whether the variation be from this cause, or from some constant variation of some fix'd stars, which hath no relation to the earth's motion; I suppose, he here means *Mallement de Mesang*, who, to evade the strength of the argument for the earth's motion, drawn from the sensible parallax amongst the fix'd stars, assigns every fix'd star to move in a small *epicycle* that will answer the appearance. (Observe only the humour and ingenuity of these great philosophers and astronomers, and judge how likely 'tis, by any means in the world, to convince such of any error they shall once assert.) Yet, be pleas'd to observe his conclusion; *viz.* but when we have found, by a great number of observations, that a sufficient number of the fixed stars have a variation conformable to this hypothesis, then we may judge that there is some foundation for it, notwithstanding some irregularity that has been, in part, observed to the contrary. The observation is extremely difficult and long, because the period of the variation, propos'd to be observ'd, is of a whole year, and requires that the instrument shall be unshakeable. It is for this, that it can no where be better done, than in the Royal Observatory. Thus far Mons. *Cassini*. To which Mr. *Ozanam* adds, [that the Royal Observatory is a haughty building, which the King has caused to be built in an eminent place, without the suburbs of St. *James's*, for making physical and astronomical observations; and that it is called Royal, for that it was built by the munificence of *Louis le Grand*, whose liberality has extended to divers persons, distinguished for their merit, and principally to a certain number of learned men, chosen out of the rest, who have endeavoured, with *Eclat*, to make sciences flourish in this Kingdom, who compose the Academy Royal of Sciences.] When my attempt first

was published, I was informed some of that assembly were angry at it, for that it had not been first thought of by them; but I confess I did not believe it. But meeting with this passage does seem to make it probable enough. However, they needed not have regretted it, since there were enough besides, as considerable to have shewn their penetrancy of spirit, and accurateness of observation; and tho' *England* possibly wants those assistants which they can boast, yet I hope to shew, that weaker means may effect many things that their more powerful have fail'd to perform, if God grant me life and health.

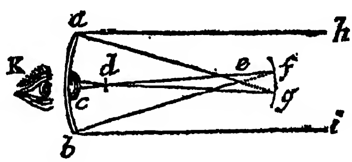
If we consider, in the next place, the fate of microscopes, we shall find much the like to have attended their performances. The first notable thing performed by it, that I have met with, was the figure of the bee made by Sir *Francisco Stelluti, a Lyncean* and presented to Pope *Urban* VIII, which is mention'd by *Johannes Faber, in Historia Plantarum & Animalium Mexicanorum*, lib. 1. p. 757. *Tam mirabilem anatomen præbuit partium omnium externarum, quæ in Ape sunt minuto animalculo, oculorum, inquam, linguæ, cornuum, jubæ, aculei, pedis, digitorum, aliarumque, & nuper in æs incidi commisit, atque felicitati Urbani* VIII *dedicavit, ut hæc omnia malim te oculis tuis intueri quam rudi meo calamo adumbrare.* And *Fabius Columna*, upon the same place, says, it was *Impressum a Lynceorum Academia S. D. N. Papæ Urbano* VIII *in perpetuæ devotionis symbolum oblatum fuit anno* 1625. *Cum nostratis apis imagine accuratissime a D. Francisco Stelluto novo quodam microscopio observata, ut qui illam viderit in admirationem incidat; tam multas partes organaque depicta discernit, quæ ab intuentium osculis in ipso animalculo omnino absconduntur.* These discoveries were also highly favour'd and practis'd by Prince *Cesius* himself, which greatly encouraged observers, and produced many in divers parts of *Italy*. Accordingly we find some observations made by *Hodierna*, in *Sicilia*, about 1640, and others recorded by *Panarolla* about the year 1650, namely, the porousness of man's hair, the red sands in urine of calculous persons, and the worms in vinegar. Many others were also found to make some few observations in other countries; but, by degrees, it is become almost out of use and repute: so that Mr. *Leeuwenhoek* seems to be the principal person

left that cultivates those enquiries. Which is not for want of considerable materials to be discover'd, but for want of the inquisitive genius of the present age.

n. d. Perhaps part of a Letter from HOOKE to Lord BROUNCKER.

Dr. HOOK's *Invention of a Reflecting Telescope*.[1]

I have lately made a telescope by reflection, with which I look directly at the object, and see it very distinct, and magnified. And this is by planting a small *lens* in the middle of the *object speculum*, and planting another small *concave speculum*, beyond the focus of the *object speculum*; the manner of which your Lordship will readily understand by the annexed scheme; where *a b* represents the object speculum, *e* the focus of that speculum,



f g a small concave speculum, serving to reflect the rays to a second focus *d*, where the eye *k* see the object by the help of the small *lens c*. 'Tis easy so to contrive the cell for the eye, that the rays that pass on each side of *f g* shall not disturb vision.

We long much to hear of Mons. *Hugenius's* opticks and mechanicks. They are subjects capable of vast improvements, and cannot be rationally expected from any more likely, than from his acute wit and excellent pen. But, my Lord, I fear I have too far trespass'd upon your Lordship's patience, and must humbly therefore beg your Lordship's pardon, and subscribe my self,

My LORD,

Your Lordship's most Faithful and most Humble Servant,

R. HOOK.

[1] Which I insert after the foregoing Papers, by reason of its congruity therewith; because I know not the time when this telescope was invented, whether before, or after Mr. *Cassegrain's*, in *Phil. Trans.* N. 83, from which it differs in some very material matters. W. DERHAM.

1692

n. d. Of Dr. DEE's Book of Spirits. *Out of the* Theatrum Chemicum Britannicum, by ELIAS ASHMOLE, London, 1652.[1]

[We do not know when this interesting review was written, but as WALLER has, in error, printed '1692' as the date of the book, it is possible that 1692 was the year in which HOOKE wrote his account of it.]

1692/3

February.

(38) *A method of examining Waters as to Freshness and Saltness.*

Phil. Trans., No. 197, p. 639, February 1692.

? *Feb. Historical evidence for Terrestrial Changes.*
[*Posthumous Works*, pp. 371–402.]

March.

(39) The passage relating to the hind wings or 'balancers' of Flies was reprinted from *Micrographia* in

Phil. Trans., p. 692, March 1693.

March 8. *Fable of the rape of Proserpine.* [*Post. Works*, p. 402.]

n. d. A Synopsis of Cubic Aequations. [R. S. MS., No. 81.]

1693

May 21. *Letter from* EDWARD LHWYD *to* HOOKE.
[MS. *Sloane*, 1039, f. 109.]

June 1.

(40) *An account of Dr. Thomas Burnet's book, entitled:* Archeologiae philosophicae, *sive* Doctrina antiqua de rerum originibus. Londini 1692.

Phil. Trans., No. 201, p. 796, June 1693.

[1] Printed in *Posthumous Works*, pp. 203–9.

July 19. *A Narration of Mr. L. L. de Vallermont.* [Of a *Magnet* made out of rusty iron that held the stone together at the top of the church of *Notre Dame* at *Chartres*. R. S. MS., No. 82.]

Nov. 29. *Of the use of Microscopes.* [R. S. MS., No. 84.]

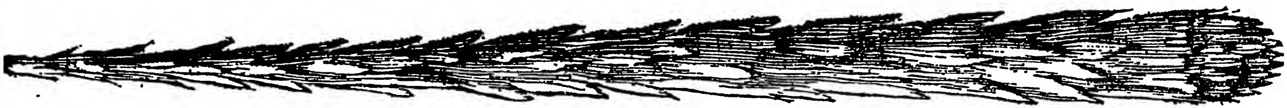
Mr. HOOKE read a discourse concerning Microscopes which was the subject of some observations by Mr. WALLER. The discourse was illustrated with the figure on p. 747. Cf. Derham, p. 270.

1693/4

January 3. *Experiments and observations about heated iron; communicated to the Royal Society.* [Derham, p. 286.]

Having lately met with some experiments which are not much known, tho' they are obvious, and easy enough to be observed and experimented, I thought it might not be altogether impertinent, nor unacceptable to this assembly, to give a short, but true account thereof; and so much the rather, because they are very pertinent for the proof and confirmation of a theory which I have formerly read before this illustrious Society, and have published in the 8th Observation of my *Microg.* for the explicating the phænomena observable about the sparks of fire, struck from the steel, by the edge of a sharp and hard flint, or some other such hard and stony body: these I found to consist of small globules, looking like melted iron, or else some small silver cut off from the steel, and thereby made red-hot, but not melted, but keeping the shape it received by the stroke, or gash of the stone; which phænomena I did there thus explicate.——'It seems that some of these sparks' &c. *pag.* 45. *line* 24, to *pag.* 46. *line* 14——'Muscovy glass.'

There are two particulars, therefore, that I have there alledged, which, by the experiments I am now to mention, will receive great confirmation. And the first of these experiments is, that two smiths, taking each his small bar of iron, both perfectly cold, and each of them hammering his bar upon the same anvil with small hammers, in a very short time, and with not very many strokes, reduced them both to so great a heat, that immediately laying them one upon another, and continuing to hammer them a very small time longer, they were thereby perfectly welded, or joined together into one piece, as firmly, as if they had



Barbed Thorn of the Prickly Pear.
(*Nov.* 29, 1693.)

been welded the common way, by being sufficiently first heated in the fire, and then hammered together. This experiment I have not yet seen tried my self, but I have been assured of the truth of it by a knowing person, who saw and examined all circumstances thereof, insomuch as I do no way doubt the truth and certainty thereof.

The other experiment is this, that taking a bar of iron, and heating it to a white heat, so that it spurts, or darts out of it every way, very shining and fiery rays; then immediately laying the same on the anvil, or a tile, and blowing the glowing iron with a pair of bellows, instead of cooling the same, as most would be ready to expect, the cold fresh air from the bellows will make it glow and burn much brighter and hotter, and will continue to do so for a considerable time; and if the bar be sufficiently heated at first, the bellows, by so blowing, will melt the same, as if it were pitch or rosin on fire. The last part of this experiment I have not yet verified my self, but the former part I have, and observed it to burn and waste under the blast of the bellows, as if it had been a piece of kindled charcoal, so blowed upon; and the flame, or light thereof, to be so very strong and vivid, that one cannot well endure to look upon the same, without much offending the eyes, as if one look'd upon the very bright face of the sun itself.

By the former experiment it is evident, that the force of the blow or stroke, which is able to cut off a sliver of hardened steel, may not only be sufficient to heat the same, to a degree sufficient to set fire on the tinder, but to intend it, so far as to make it of a welding or white heat, which having acquired, and flying off into the air, with a very quick motion, by the 2d experiment, 'tis evident that the operation of the air is sufficient to intend the heat yet further, so as to melt, or vitrify the same, and thereby to cause it to be formed into a globule, ball, or shell, as it often appears through the microscope. All which effects are more easily perform'd on so small a body, as are those slivers which are struck or cut off from the hardened steel. But the globules, balls, or shells, that are made by the melting of the heated iron, blown on by the bellows, are much bigger, and more conspicuous, but of the same form and substance. Nor is this combustibility peculiar only to iron, tho' therein it be very notable and con-

spicuous, but the other metals have also their combustibilities in their distinct kinds, as copper, brass, lead, tin and silver; upon each of which the menstruum of the air will work and dissolve, or burn them when they have first been prepared by a proper degree of incallescency, as I shall, at some other time, make manifest, by plain and evident experiments.

1694

Dr. HOOK'S *account of* Mons. DE LA HIRE'S *discourse of Frost.*

I have perus'd the book of *Dan. Bartoli*, concerning frost and ice; and tho' he hath many arguments to destroy the sentiment of several of the moderns on that subject, of *Valesius, Des Cartes*, Mr. *Boyle, Olaus Magnus*, Sir *Kenelm Digby*, &c. yet I do not find any other doctrine affirmed concerning it, but that he conceives it done by a nitrous substance, which is of a cold and dry nature, which operates after the same manner in coagulating the water, as the runnet doth in coagulating milk; but, how that is done, I do not find he does explain.

Having therefore fail'd of my expectation from him, I resolved to see what satisfaction I should have in perusing a discourse, upon the same subject, of a much newer date, namely, that of Mons. *De la Hire*, publish'd at *Paris* in 1694, whereas that of *Bartoli* was publish'd at *Rome* 1681. This I found to be much more concise, and plain, and positive in what he has deliver'd, and much more clear in explicating of his notions and conceptions of it; so that tho' I could not meet with such an information concerning ice and frost, as I could have wished, yet in perusing 16 pages in *quarto*, which is the whole treatise, I was satisfied that I understood fully what he intended to communicate; whereas I was to seek, what was intended by the other, in almost ten times the number of pages.

Mons. *De la Hire* then begins his discourse, by defining or explaining what he means by cold; that is, the sensible quality in frost; and this, he says, is nothing else but a less agitation or motion of the aqueous particles, whether blended with the air, or united in a mass, than of the like particles in the skin, or pores and vessels of our body. He might, he says, have added another assertion, that all the agitation of aqueous particles proceeds from

that of the subtle air; but he thinks it sufficient, for this discourse, to shew how all the phænomena of cold will be plainly solved, by the explication he has premis'd.

Next, he says, that these particles are depriv'd of their motion by certain particles of salt, which are very minute, long, stiff and sharp, which, by their motion, are easily carried and blended with the air, but do more easily insert themselves into the particles of water, than of any other body, nay, even than of the salts themselves, from whence they proceed, which he reckons to be from common salt a little, from niter more, but most of all from *sal amoniack*, which salts do therefore easily dissolve in water; that these particles do penetrate metals, and even glass, but that they are most entangled, and stay'd by the particles of water, which he supposes, with *Des Cartes*, to be long and flexible, like strings or threads, and by that means they destroy the motion or fluidity of each other, which composes a solid, hard and dry body, which is ice. This ice, he says, encreases extension, by means of these salts, and so breaks the vessels that contain'd the water; and, being so extended, is lighter, and so floats on the water. Hence 'tis, he says, that blebs are form'd in the ice; but his explication of this phænomenon is not consonant to the other suppositions. By the bye, he explains the expansion of water by heat, and that he makes to proceed from the expansion of the parts of the air contained in it. Hence he concludes, that there is a middle state of the water, which is its specifick expansion, and it is then cold, because tho' it may be easily moved, and so retain motion enough to keep it fluid, yet it has so little motion of its own, that it communicates none to other bodies. The reason, why oils and spirits freeze not, he says, is because they have few of those entangling aqueous particles: by this he gives a reason of the not freezing of other bodies. To fortify this hypothesis, he explains the experiment of the expansion of spirit of wine, by the application of snow; and freezing other bodies by application of niter and *sal amoniack*, and spirit of wine; and, by the way, he tells a pretty method of cleaving mill-stones, by the swelling of small wooden pins, drove into certain holes, drill'd in a line on the stone where 'tis to be cloven. He takes notice also, that the refraction of ice is less than that of water, and quotes his own

publication of it in 1693, though it was shew'd by me, to this Society, 30 years before; (but 'tis not usual for those writers to own discoveries to be made by any but themselves, who take themselves to have an Empire over all the rest of the world). He also takes notice of several other phænomena, and experiments of ice and frost, mentioned by Mr. *Boyle*, but without naming him. Upon the whole, I conceive, he has more particularly applied the *Cartesian* notions of particles, motions, figures, *&c.* to the explication of these phænomena of cold: but as the supposition of such qualified particles is wholly precarious, so neither will those, without a great many other supplemental suppositions, suffice to solve the phænomena satisfactorily, unless the particles be supposed to act and operate by instinct; and tho', possibly, they might serve to put a seemingly probable explication of these phænomena of cold, by supposing them thus, or thus, qualified and adapted; yet I very much fear, there are some phænomena of heat, or of other qualities, wherein the same particles of bodies are necessary to be introduced as the principal agents, tho' their actions in those be quite contrary to their actions in these. It would be, therefore, but a second lost labour, to shew that these particles are of another nature than what they are here supposed, and to assign them other figures, motions, and qualifications: because, first, it would be almost as much labour to demolish this fabrick, as it was to raise it, and a third fruitless labour to erect another. Nor can it be expected to be otherwise, till such a structure be founded upon a natural, firm, and solid ground, and not upon feigned and imaginary suppositions.

July 25. HOOKE'S *criticism of* DE LA HIRE'S *observation* on *a Petrifaction.* [*Posthumous Works*, p. 447.]

Dec. 5.
Dec. 12. HOOKE repeated his Lecture of Dec. 3, 1690, on the *Use of Scientific Instruments for Navigation*, q. v.

History of Thermometers. A new Sea-Quadrant.
[*Posthumous Works*, p. 555.]

Tab: X.

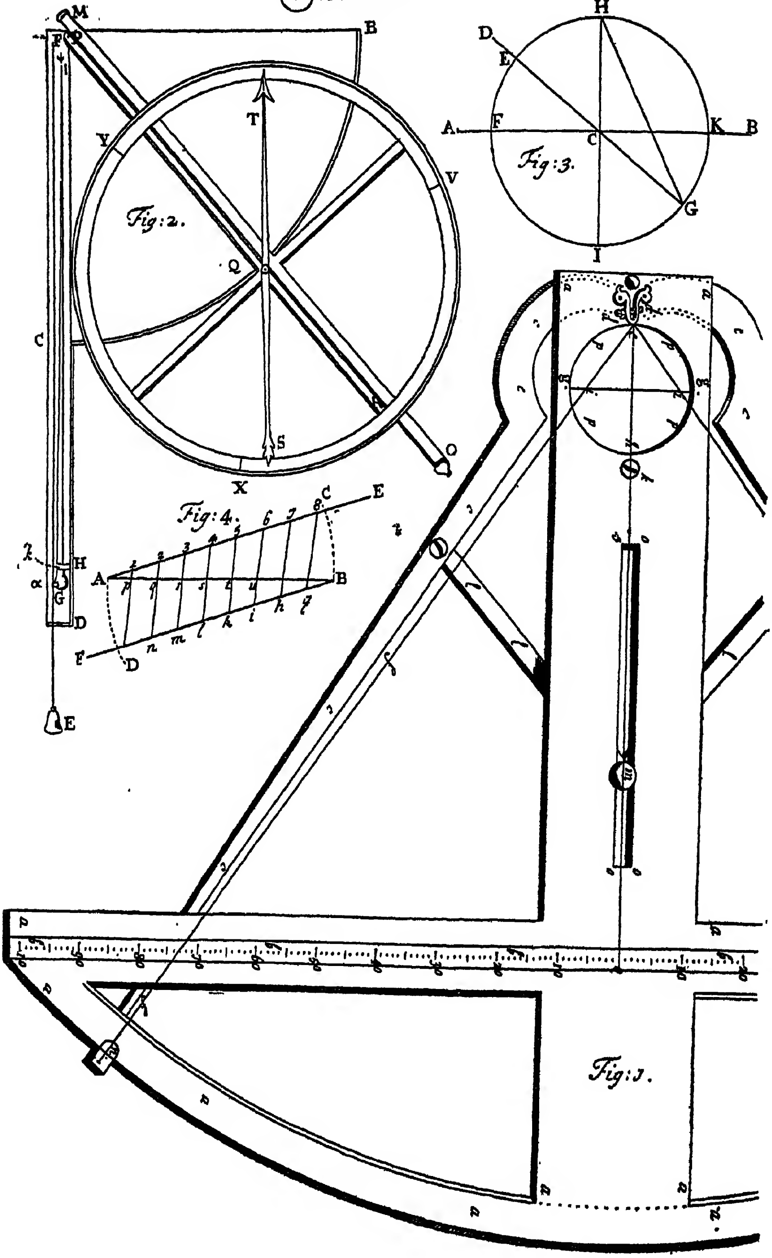


Fig. 1. "The Divider for Hevelius": a Micrometer for measuring the minute Distances of Objects at the focus of a Telescope.
Posthumous Works, p. 498.

Fig. 2. Small Quadrant to observe Altitudes and Angles of Stars with precision. *Posthumous Works*, p. 508.

Fig. 3. Diagram to illustrate the relation of angles to the arcs of a circle.

Used to illustrate Hooke's Lecture on Navigation of Nov. 19. 1685.

Fig. 4. Scheme for dividing a line into any given number of equal parts.

Dec. 19.

An instrument of use to take the draught or picture of any thing.
Communicated by Dr. HOOK to the Royal Society.

Among the instruments that may be of use to curious navigators and travellers, one is, for procuring the pictures, draughts, or true forms and shapes of such things as are, or may be, taken notice of by them; that is, not only of the prospects of countries, and coasts, as they appear at sea from several distances, and several positions; but of divers in-land prospects of countries, hills, towns, houses, castles, and the like; as also of any kind of trees, plants, animals, whether birds, beasts, fishes, insects; nay, of men, habits, fashions, behaviours; as also, of all variety of artificial things, as, utensils, instruments, engines, ships, boats, carriages, weapons of war, and any other thing of which an accurate representation, and explanation, is desirable. For, tho' a description in words may give us some imperfect conception, and idea, of the thing so describ'd; yet no description, by words, can give us so full a representation of the true form of the thing describ'd, as a draught, or delineation of the same upon paper. Nor can we so perfectly conceive, or imagine, the true colours, by words, as by seeing the very colour it self imitated and compared with the life, or the real thing: whence we find how imperfectly the colours of plants are represented by herbals, which are wash'd, or colour'd, only from the descriptions which are made of those colours in the books.

Now, though this be not a new design, or a thing that has never been done before by any that have given us accounts of

their travels; yet, if we do but consider, how the most of those have been done, it will, I conceive, make this, which I propound for this effect, so much the more valuable. 'Tis well known, that the books commonly made for the use of seamen, (now commonly called *Wagoners*, because one *Wagoner* printed a collection of many such observations) that these books, I say, are full of the prospects of countries, as they are said to appear upon the sea, at such distances and in such positions: and I lately saw a book containing the prospects of all the western coasts of *America*; but any one, that understands prospect, will easily discern, how rude, imperfect, and false a representation, all such books contain of the places themselves: for, not to mention the impossibilities they often represent, as the over-hanging of mountains for half a mile, or a mile, which, tho' the mountain were made of cast iron, were impossible to be sustain'd in such a posture: the extravagant heights they generally raise the hills to, and the sudden and very decline descents they make them have into the vallies, do plainly enough demonstrate them to be no true representations of what they are design'd for. And, indeed, they are most made by the hands of the mariners, who are, generally, very little skill'd in the art of delineation; and, therefore, 'tis not to be expected that they should be very exact: however, even these are of very good use for navigators; and they furnish them with a better idea of the appearance to be look'd for, than descriptions by many words would inform them. Again, we find that many relations of foreign countries do give us pictures of towns, prospects, people, actions, plants, animals, and the like; and those beget in us ideas of things, as they are there represented. But, if we enquire after the true authors of those representations, for the generality of them, we shall find them to be nothing else but some picture-drawer, or engraver, here at home, who knows no more the truth of the things to be represented, than any other person, that can read the story, could fancy of himself, without that help. Such are all the pictures in the books of *Theodore de Brie*, concerning the *East* and *West-Indies:* such are also the greatest part of the pictures in Sir *Thomas Herbert's* travels; and those of Mr. *Ogylby's Asia, Africa,* and *America*; which are copies of the *Dutch* originals, and are, originally, nothing but Mr.

Engraver's fancy: so that instead of giving us a true idea, they misguide our imagination, and lead us into error, by obtruding upon us the imaginations of a person, possibly, more ignorant than our selves.

It is, therefore, the interest of all such, as desire to be rightly

and truly informed for the future, to promote the use and practice of some such contrivance as I shall now describe; whereby any person that can but use his pen, and trace the profile of what he sees ready drawn for him, shall be able to give us the true draught of whatever he sees before him, that continues so long time in the same posture, as while he can nimbly run over, with his pen, the boundaries, or out-lines of the thing to be represented; which being once only truly taken, 'twill not at all be difficult to add the proper shadows and light pertinent thereunto. By the same instrument also, the mariner may very easily and truly draw the

prospect of any shore, and from time to time denote the rising thereof, as he does nearer and nearer approach it, and the depression, or sinking of it, as he does recede.

The instrument I mean for this purpose, is nothing else but a small picture-box, much like that which I long since shewed the *Society* for drawing the picture of a man, or the like; of the bigness of the original, or of any proportionable bigness that should be desired, as well bigger as smaller, than the life; which, I believe, was the first of that kind which was ever made, or described by any. And, possibly, this may be the first of this kind, that has been applied to this use; tho', upon the first institution of the Royal Foundation of *Christ-Church*, I propounded it to the Governors there, for the use of the children: but Sir *Jon. More* undertaking to write an institution, and having omitted it, it has not been there brought into use.

1694/5

Feb. 13.

A way to measure heights and distances, &c. at sea.

[Derham, p. 296.]

That, which I shall at present explain, is a method of measuring the bearing and distance of objects seen at sea, such as ships, or shores, islands, promontories, castles, towns, mountains; their heights, as well as distances: also the course, length, breadth, *&c.* of rivers, and the like: as also, for knowing the distance from any light, or light-house, seen in the night. Now, tho' experienced navigators do, by long practice and use, give pretty near guesses at them; yet the way I shall propound, I conceive, will come much nearer, and be much more certain, and may easily enough be put in practice; which if the gentleman, that describ'd the coasts of *England*, had known, or put in practice, I conceive, he would have prevented many mistakes he has therein committed. However, tho' it be now too late for that purpose, yet it may be of good use for such as may attempt the amendment of those, or any other coast-maps, or charts, for the future. And I have the rather mentioned it at this time, for that somewhat of that kind is shortly design'd to be undertaken. And it would be, as I conceive, very much the interest of all mariners, merchants, nay

States that are concerned in maritime affairs, to be at a constant charge to have such a design prosecuted, till it be compleated for the whole world, at least for all coasts that are traded to, or much frequented, or which are often passed near, or touch'd at, in farther voyages; that seamen, in case of distress, might know where to find convenient harbouring, and also accommodations of fresh water, wood, victuals, *&c.* I know the work is great; yet it is necessary, and ought to be done, some time or other, and therefore the sooner the better. Somewhat of this kind, I know, is accidentally done almost by every navigator, and recorded in their journals; but most of those being kept by themselves, they are of little publick benefit, and serve only for their own future information. But those who have made it their business to collect and digest such journals, and to print the results thence deduced, which the *Hollanders* and *English* have prosecuted more than any nation besides, have very much deserved the acknowledgments of all the rest of the world; as all such for the future will do, who shall promote and encourage such a work.

The way then, which I propound, is perform'd by taking the true bearing of an object at the same instant from two stations, which, the farther they are removed from each other, the more fit they are for this purpose. Now, because both these stations are to be comprised within the ship, or vessel, made use of, I would have them to be, at the extremities, of the length of the vessel, to wit, at the stern and head, or in the round upon the head of the boltsprit, which will add somewhat to the distance of the two stations; for, upon the measure of that depends the measure of all the other lengths or distances. Now, in each of these places which are pitched upon for the stations, I would have a fix'd frame, or pedestal, for the holding of the instrument to be used on it, and the instrument so fixed to it, as to remain firm and steady in any posture desired, and yet, with the greatest ease imaginable, so to be moved, as to respect directly the object requir'd, and, when the observation is made, to be as easily removed, and as easy again to be fixed. The instrument I would have to be sextants of about two foot radius, most exactly graduated; on each side from the middle line, that is, to 30 degrees on each side, and to be fitted with perspective sights,

whose *rete*, or sight-point, shall always be in the center of the instrument, and that center always in the line and *terminus* of the distance of the two stations, which shall be invariable, however the instruments are moved to respect the objects; to which purpose each of the instruments shall have a double motion; one of which shall be exactly upon the line of distance of the instruments, whereby the plane of the whole instrument is moved; and the other of the sight, upon the plane of the instrument it self, so as to respect the object, and give the angle that the line of the sight makes with the former axis of motion, or with the middle line of the instrument; which middle line ought to be exactly perpendicular to the axis of the motion of the plane of the instrument, which is the line of distance. Next, there should be two expert observers placed to make use of these instruments, and each of them, at the same instant, should direct his proper sight to the same point of the object; which, that it may be done the more exactly, I think it convenient, especially in large ships, to have a line, packthread, or wire, to pass between the two observers, by which they may, at the instant they desire, advertise the corresponding observer, of what will be necessary, according to the signs or directions they have before mutually agreed upon. By this method, if well executed, I do not doubt, but that heights, distances, and positions of objects, seen on the sea, may be estimated ten times more exact than any that are now made by judgment, (as they say) or rather by guess. And, if any one will endeavour to put it in practice, I shall be very ready to explain any part thereof more fully, and particularly, for his information.

1695

Method of drawing Arcs of Large Circles.

May 16. At a meeting of the Royal Society Mr. Hooke discoursed of his instrument to draw a great Circle by the direction of a wire about a hundred foot long.

May 22. The same subject continued. The apparatus included two rouling Circles or Truckles in the two ends of a rule, made so as to be turned in their sockets to any assigned angle.

[*Posthumous Works*, pp. 531–4.]

Water supply.

June 1. I stayd with Mr. BLOUNT and Mr. HALLY from 8 to neer 12 a'clock, then went and dined at the Roman in Queen Street, with Sir CHRISTOPHER WREN, Mr. BLUNT and his cozen Mr. ALDERSEY etc., about 4. We measured the water that vented by the wast pipe, and found that it filled a barrell in 2′ 15″. This Mr. HALLY, Sir CHR. WREN's clerk and I observed 4 times. Twice we judged 2′ 5″. Twice more we judged 2′ 20″. Then we measured the water at Fleet Bridge and found it fill a Barrell in 50″, 51″, 52″. We agreed on 50″. Hence we went to changes heald (?) and Sr CHR. WREN and I both saw the surface of the water in the cisterne there 6 inches below the pipe that brought in the water. From thence we returned to Charing Crosse and found it fill the same barrell in 45″, 46″, 48″, 48″. Thence I returned home.

[HOOKE's *Diary*, MS. *Sloane* 1039.]

June 20. *On Arcs of Large Circles.* [*Posthumous Works*, p. 534.]

July 3. Extract out of the Parisian Academie's Memoires relating to the *Alteration of the Axis of the Earth's motion.* Read to the Royal Society on July 3. [*Posthumous Works*, p. 536.]

Nov. 6. *Letter from* JOHN AUBREY *to* HOOKE.

[MS. *Sloane* 1039, f. 108.]

Dec. 17.

DR. HOOK'S *contrivance to augment the divisions of the Barometer, in a discourse to the Royal Society.* [Derham, p. 302.]

The following contrivance I met with in a small script of paper, and find it was a part of a larger discourse on the subject, which never came to my hands. W. DERHAM.

The other by a counterpoise and wheel, whereby I could make an index point the divisions of a long spiral line, not only of one revolution of that line, but many whole revolutions in a spiral line: so that if one round of the spiral were six foot compass, and so easily susceptible to 1000 divisions, I could easily make it move six or eight revolutions, each of which should be equally capable of the like number of plain and very visible divisions, which maketh the difference of two inches in the common, to become 40 or 50 foot in this, and consequently capable of eight or ten thousand divisions, as sensible and plain to be seen, as the half decimals of an inch; and the contrivance is such, that there is no manner of stiffness or rubbing in the contrivance, but each

of these divisions will be as exactly pointed to by the index, as the index, in the common single barometer, can be pointed to by the surface of the mercury; which, since it is usually comprised within 40 decimals, or parts of an inch, or two inches, and this way it may be made 40 or 50 foot; it follows, that consequently the alterations will be 200, or 250 times more visible and discoverable, than by the common barometer.

And having brought it to this pass, that I could, by these methods, be able to make the smallest alterations, (that have yet been imagined) to be sensible and measurable, I desisted from improving this subject, by further contrivances upon these principles. However, I may, in time, shew some other instruments for discovery of the weather, that may, come to be of as good use.

1696

June 3. Abridgement of HARTSOEKER'S *Physicks* and of DE LA HIRE'S *Of Frost and Cold.*

July 8. Abstract of WHISTON'S *Theory of the Earth.*
[R. S. MS., No. 85.]

Dec. 2.
DR. HOOK'S *conjectures about the odd phænomena observable in the shell-fish called the Nautilus.* Read to the Royal Society.
[Derham, p. 304.]

For the right understanding of this matter, I shall give a brief account of this animal from Aristotle, Pliny, Oppian, Ælian, Bellonius, *and their transcribers,* Gesner, Aldrovand, *and* Jonson, *viz. that the* Nautilus *is an inhabitant of the deep: that it hath three motions,* viz. *a power to raise it self up from the bottom to the surface of the sea; that it can sail thereon; and again sink itself to the bottom: that its shell is made very commodiously for these three motions, with divers cells: that it can erect its shell edge-ways for sailing: that it hath two (some say three) arms, or claws, with a thin and light, but strong membrane between them, like that of palmiped birds: that this it hoists up and spreads like a sail, and is driven thereby on the surface of the sea: besides which, that it hath also other parts on each side of it, that it lets down to steer and guide its course, as with a rudder, so long as no danger is nigh: but, if it perceives any*

danger from the more powerful animals, or storms, that then it fills its shell with water, and suddenly sinks itself to the bottom.

But for the reader's diversion, if he hath a mind to see Oppian *the Poet's elegant description of this inhabitant of the waters, as translated by* Lippius, *he may find it thus in* Aldrovand. de Testaceis, *l.* 3. *c.* 5. [Quotation omitted.]

The account which Dr. HOOK *gives is thus:*

The structure of the shell of the *Nautilus*, which as it is very curious, and indeed very wonderful, so it is not less instructive to one that shall contemplate on it; and to me, as yet, it appears to be the only instance of a contrivance truly wonderful; for that I do not know any thing like it in the whole genus of fishes, tho' there are some instances that tend that way. It is, in short, this: the creature, it seems, to whom this shell is adapted, by accounts we have of it, is an inhabitant of the Abyss, or Great Deep; which how deep it is none yet knows, nor will know, till some of my *Nuntii ad Abyssum* (which I have formerly acquainted you with) be sent thither, and bring back tidings concerning it; or, till this our present *Nuncius* can find a way to manifest, how far he has ascended to come up to the day, or how far he descends to go to his resting-place at the bottom of the sea. For these progresses he is said to make, besides his voyage, when he sails on the top of the ocean. Now being constituted by nature to perform these, and yet to be without wings or fins, to help himself by labour to move in any of these three ways; it is wonderful to consider, by what a plain and easy contrivance the All-wise Creator has endowed him with sufficient faculties to perform the same, with very little or no fatigue at all, but to be carry'd in his chariot, or rather ship, from place to place, as he has occasion to change his residence.

The manner of which (if I am not mistaken in my conjecture) is this: nature has furnished him with a curious shell, dividing it into many distinct cells or cavities, by certain valves, diaphragms or partitions, which have no communication with each other, but only by means of a gut or *ductus*, which passes through them all from the bowels or body of the creature, placed in the cavity of the mouth of the shell to the very end of the spiral cone, or

conical shaped shell, which ends in the very center or beginning of the proportional *spira*, and has there a *spiramentum* or vent, which I have formerly discover'd, by examining more curiously one of that kind, by opening it, though it has not hitherto been taken notice of by any author that I have met with. The axis, or middle line of this cone, or conically-shap'd body is spiraled round exactly in a plane, and not helicated on a conical surface, as in almost all the shells of other the conchylious fishes, it is observable. Now this admirable structure seems to me not a mere *lufus naturæ*, or a form by chance, to express, a variety, but an emanation of that infinite wisdom, that appears in the shapes and structure of all other created beings, which is to endow them with sufficient abilities to perform those actions, which are made necessary to their well-being. Now, the relations of histories of this creature inform us, that it has three kinds of motions through the water, that is, ascending, descending, and progressive; and since there is one posture of the shell, that is most proper to perform each of these, therefore it is, as I conceive, that the shell is so contriv'd, as to be put, and kept in that position, whilst it performs that motion: the shell then is contriv'd to be all a cavity, and to have no other part or bowel of the creature within the first cavity, but only a small string, gut, or *ductus*, which passeth from the body of the creature, placed in the mouth of the shell, to the end of the conical cavity. Now by this I conceive, that when this cavity is fill'd with water, the whole bulk becomes heavier than the water, and so must sink to the bottom of the sea: but when the cavity is fill'd with air, then the whole will be boyant, and lighter than the water, and so rise to the top, and float on its surface: these powers it would have had, supposing the cavity of the shell had had no other but the first or greatest diaphragm, and the rest had been one entire cavity: but this would not have disposed the shell to all those motions, it is to perform, into the most convenient postures; for that posture, that is fittest for its rising, would not be so for its sinking, nor for its sailing, nor possibly for its progression at the bottom, (if such a motion it does perform, as to me it seems rational enough to suppose) for that every one of them will need a different posture. We find, therefore, this cavity all subdivided by internal dia-

phragms or partitions, into a great number of distinct cells, (I have found 40 in some shells) and every one of these penetrated by this gut or *ductus*, so that by means thereof, I conceive, the animal has a power to fill or empty each of those cavities with water, as shall suffice to poise and trim the posture of his vessel, or shell, fittest for that navigation or voyage he is to make; or if he be to rise, then he can empty those cavities of water, or fill them with air which lie toward that side, that part the shell, that best penetrate the water: if he be to descend, he can fill those with water, and empty the opposite; if to sail on the top, he can evacuate those cavities that will trim his shell fit to sail with the mouth of it upwards, that he may there expand his sails and use his rudders; and if to move at the bottom, he can fill those, and empty the opposite, so as that the mouth may be downwards, to respect the ground or bottom over which he passes, so to discover his proper nutriment or other convenience, and to descend to it when he finds it. Now it may be imagined, and objected, that these operations may be too notional and fanciful, and so seem to have more of design and counsel, than the creature seems to be capable of: to which I answer, that it is no more, nor, may be, so much, as most other creatures are endow'd with, and constantly perform: for whoever considers what design and contrivance there is for the performance of all muscular motion, where this or that muscle is to be trained, and that or the other muscle is to be relaxed, and presently the quite contrary effects are to be effected, and all these to proceed from the will, or intention of the creature that moves himself thereby, which way it pleaseth, will not think it so strange to conceive, that this creature may have implanted in it a faculty, to make use of the organs for motion, as well as any other: there needs no institution of a bird to make use of his wings to fly, or of his tail, to poise or guide him in his flight; no, nature, or the infinitely wise God of nature hath taken care to give him an instinct or impulse, which enables him to do those things, that are necessary to be done, for the producing the desired effect. Now, though the shaping, and trimming, and steering of an artificial ship, doth require the understanding of the men that are to act in that ship, to know, and accordingly to dispose of all things, for the effecting what is necessary or

desired; yet 'tis not thence to be argued, that the operations of animal motions must be perform'd by the operations of reasoning. No man can tell how, or by what means, he moves his finger, or any one muscle of his body; no, nature hath set all things in order, and endow'd us with a power to perform what is necessary, though we know not how, nor by what means; nor is the notion, I have hinted, so extravagant, or so much beyond the other contrivances, for the effecting of various motions in other animals, as some may imagine, since, when I come to treat of that subject, I shall shew, and prove several contrivances, that are actually made use of, that are abundantly more wonderful.

Dec. 3. *Letter from* TH. CROWLEY *to* HOOKE.
[MS. *Sloane* 1039, f. 126.]

n. d. Analysis of MARQUIS DE L'HOSPITALL'S *Infinitely small quantities for the finding of Curves*, printed at Paris, 1696.
[R. S. MS., No. 87.]

n. d. Account of DR. BERNARD'S Treatise of Weights and Measures.

n. d. Concerning LEEUWENHOECK'S letters and improvements in the *Microscope*. [R. S. MS., No. 89.]

Dec. 16.

[Dr. HOOK resum'd his considerations of the Nautilus, and having taken notice of several transmutations, as particularly of water into the solid parts of vegetables, as also into earth or ice; he then proceeds, and saith,] W. DERHAM.

But this metamorphosis, or transmutation of elements, I take notice of here, only by the by, as it may be of some use for the explication of another metamorphosis of a contrary nature, and that is, of water into air, which is by rarefaction, for such an operation nature seems to have; and somewhat of this kind is producible by art, as has been prov'd to this Society by many experiments, heretofore made, for the production of artificial air; which, though under that notion it seem'd not to be regarded, yet, as such another, published a good while after all those experiments, as his own, not owning at all he had been inform'd of them, by some of the members of this Society: but to pass by that at present (because there are abundance of instances of the like nature that have been given, which I may on some other

occasions manifest) I had a further prospect in the success of those trials than what was, for the like reasons, then spoken of; one of which was, for the solution of such a phænomenon as this, of the floating and sinking of the *Nautilus*, which I discoursed of the last meeting but one. It seem'd, indeed, very strange, how that creature could do so, at his will fill, and empty, the cavities of his shell, with water; it was easy to conceive, how he could fill his shell with water, and so sink himself to the bottom; but then how (when there, at such a distance, from the air) he could evacuate the water, and fill the cavities with air, that was difficult to comprehend, especially being under so great a pressure of water: but if nature had furnish'd him with a faculty of producing an artificial air, then the riddle would quickly be unfolded. I found, therefore, that by art it was feasable to produce such an artificial air, and that it was endued with a very great power of expansion, so that it would not only make itself room to expand, notwithstanding the incumbent pressure of the air on all sides; but, if sealed up in strong glasses, it would break out the sides there of, which might have as much power of expansion as might counterpoise, nay, out-power both the pressure of the air, and also the water too, though 100 times greater than that of the air. It will be, I confess, a difficult matter for me to prove, that the *Nautili* have such a power, for that I could never yet get a sight of that fish that inhabits those shells, nor do I find that any of the authors, that pretend to describe it, have, nor has any of them given a description of it that can give one any true idea of it: yet, methinks, it might be procured from some ingenious person, that has an opportunity of visiting the *Barbadoes*, and some of the other Leeward Islands, where there are found great plenty of a smaller sort of them, which though of a differing shape, in the coil of the conical body, yet they agree with all the other kinds of them in having the diaphragms, and a *ductus*, or vessel passing through them all, from the basis to the apex of the coiled cone, and the axis of that cone is also coiled in a plane, as are all the other kinds of the *Nautili*; of which I have one here to shew, given me by one who had a whole box full of them, which he had there collected, and brought with him to *England*.

Dec. 23.

Some farther observations relating to the Nautilus, and other shell-fish.

I explain'd, the last day, the fabrick and structure of a creature, which, as authors inform us, is an inhabitant of the abyss or great deep, which does often perform a voyage from thence to this superior region of the air; and, after the dispatch of his business here, returns again to his own habitation. I explain'd also, by what method he perform'd these voyages, as I conceiv'd, from the consideration of the structure of the shell, and the effects perform'd by it. I cannot be positive in it, as not having ever had an opportunity to see the creature itself: but by considering of the contrivance of other fishes, to help them to float in the water, or at least to buoy them up, or counterpoise them with the water, by the help of the *swim*, as 'tis call'd, or bladders blown up by air, or vapours, I think there is a great probability in the conjecture.

For the *Nautilus* is not the only inhabitant of the deep, or of the bottom of the sea; no, questionless,, there are a multitude of other sorts of animals that are there bred, and do there reside; for we do not only find oisters, scalops, cockles, periwinkles, and most other kinds of shell-fish, but most sorts of crustaceous animals, as various sorts of lobsters, various sorts of crabs, and various sorts of prawns or shrimps, and such like; nay, we find there also several sorts of fishes, not furnish'd either with shells, or crusts, which the fisher-men always find and catch, near the bottom of the water, where they fish for them: and I myself have proved, that the best place, to lay the bait to catch whitings, grundells, place, flounders, beards, is at within a fathom of the ground, where the depth of the sea was about 25 fathoms, or 150 foot; and, from as great a depth, I have known lobsters and crabs to have been taken by the same fish-hooks, which were baited for the catching those other sort of fishes: and, indeed, most part of the lobsters, crabs, and prawns, are taken, in fish-pots, or fish-cages, laid at the bottom of the sea, when there has been found a place frequented by them: as also scates, thorn-backs, monk-fish, dog-fish, and the like, which are catched by baited fish-hooks, laid at the bottom of the sea, they being all

ty'd by strong short lines, ty'd to a rope, there extended between two stones, which there keep it extended. So that most fish, of all kinds, do, for the most part, there reside, and thence it is probable to conjecture, that there they find the greatest part of their food and nourishment, and that there do likewise grow abundance of distinct sorts of vegetables, which may be useful for that end; for we find, in seas that are not very deep, that divers sorts of algas, sea-phans, sponges, cotulli, and the like, are there produc'd; and why then may there not be multitudes of others? Nature, we find, does accommodate every thing it produces with all conveniencies, necessary for its support and well-being, and fit every thing necessary for the carrying on and perfection of its designs; so that I see no reason to doubt, that these sub-marine regions are as well stock'd with variety of animals and vegetables, as the surface of the earth, which is only sub-aerial, only we are less knowing of them, because they are out of our element, and we want *nuntii* or messengers, to send thither to bring us back information, and also the productions and commodities that this *terra incognita*, or unknown world, does afford. I have heretofore produced some such *nuntii*, for this or that particular design, but when there may be an opportunity of sending them, I shall be able to produce divers others, for other purposes, if God spare my life so long as to see the seas again free from rovers, and that the study of arts does succeed the study of arms. It is now about thirty years since I try'd many experiments, for this very end, to know under how great a pressure a terrestrial or aerial animal could live, and consequently a man; and I shew'd a way also how to supply him with fresh air from above, to whatever depth he should be able to descend, without prejudicing his health or life: I shew'd also how to accommodate him for seeing with spectacles, and acting freely in the water as he could do in the air, by means of other accoutrements, whenever he was able to endure the pressure. And I have many other experiments, which would be not only instructive, but useful for these and other designs, but I want an apparatus and assistance to perform them. And, probably, most people will treat me as *Columbus* was, when he pretended the discovery of a New World to the westward: but I have been

accustomed to such kind of treatments, and so the better fitted to bear them. However, I think, that such objections as most will be apt to make, that animals and vegetables cannot be rationally supposed to live and grow under so great a pressure, so great a cold, and at so great a distance from the air, as many parts at the bottom of very deep seas are liable and subject to; I say, I think that these objections may be easily answer'd, by shewing, that they all proceed from wrong notions that men have entertain'd, from the small experience they have had of the effects, and powers, and methods of nature, and a few trials will easily convince them of the erroneousness of them. We have had instances enough of the fallaciousness of such immature and hasty conclusions. The torrid and frigid *zones* were once concluded uninhabitable; and to assert *antipodes* was thought atheistical, heretical, and damnable; but time has discover'd the falsity and narrowness of those hasty conclusions.

[W. DERHAM.]

1696/7

Dr. HOOK'S *discourses to the Royal Society, in the beginning of* 1697, *concerning Amber.*

The sum of Dr. HOOK'S *opinion, in these discourses,* Mr. WALLER *gives in this following Preface,* viz.

Having met with a treatise concerning *amber*, publish'd by ——— (of which he gave an account) he proceeds from several observations therein mentioned, and some of his own, to give his own sentiments, *viz.* that *amber* being found almost all over *Prussia*, as well in the inland parts, as in the sea, on the shore, in the caverns, clifts, and under the hills, by digging, and this in a sort of *minera arenaria*; which, by the substances found in it, such as shells petrify'd, and the like, Dr. *Hook* judges to be a certain layer, or bed of sea-sand, the remains of the bottom of some sea that formerly covered the whole country, which, in process of time, has been raised above the level of the present sea; but, at a certain depth, all that sandy bottom yet remains, containing such substances as were there deposited, whilst it was in that state; at least, such of them as have not been rotted and consumed by time, such as petrified shells, wood, bones, with

vitriol, alum, niter, and sea-salt, together with lumps of *amber*, are frequently now found in digging into this sand, for wells, or the like. Here he has recourse to this hypothesis, formerly discoursed of, for the solution of these appearances, *viz*. that not only the vales, and lower parts of the land, have been some time the bottom of the sea, but even the tops of hills and mountains; as the several substances now found thereon make evident. *Amber* then being thus found, either at the bottom of the sea adjoining, or in these layers of sand, the question is, how it came there? and from whence? To answer this inquiry, tho' the author of the treatise is of another opinion, yet, from several observations therein mentioned, Dr. *Hook* judges it to have been the gum of a certain tree petrified, and altered to the present state and appearance it has. Thus far Mr. *Waller*; next follows,

Dr. HOOK'S *discourse of* Feb. 24, 1696–7.

I have lately ventured to assert my opinion, that *amber* is a kind of petrified resin, or the exudation of some resinous tree, concreted into a substance so much seemingly different from it, that most of the authors that have treated of it, or described it, have been quite of a different opinion. Nay, even the last, and, I think, much the best, that is, *Philippus Jacobus Hartman*, who has publish'd a tract, intituled, *Succini Prussici Historia Physica & Civilis*. For, after he has disproved, as he conceives, all the opinions of those who have writ of *amber*, and, amongst the rest, those of such as have inclin'd to think it originally some vegetable substance, *&c*. he thus concludes, *p*. 16. of his *2d book*. *Subterraneum utique succinum apud omnes in confesso esse, idque ex historia satis probari; cum vero, id nec duci nec fundi possit, metallis non accensendum esse, neque ex reliquis fossilium generibus terris, sulphuri aut bitumini anumerandum, quod soliditas succino major quam quæ ejusmodi fossilibus inest: lapidem igitur reliquum esse, ut dicamus, & quidem non ex saxorum aut marmorum, sed nec ex lapidum peculiariter ita dictorum genere, sed gemmam, per quam apte responderi possit ad quæstionem, quid sit cum naturam ejus recte exprimat*. Now, how much the wiser we are, as to the knowledge of its nature and original substance, I leave to others to judge; to me, I confess, it seems more obscure,

than if he had said that *amber* is *amber*; for, what he understands by *gemma*, to me seems more obscure. He has, indeed, many pertinent relations, and observations, which have much assisted me in my inquiry; but the uses and inferences, he draws from them, are quite contrary to those which I have remarked them for. 'Tis not my design to contradict his opinion, or to make objections to his doctrines: I think it fairer to propound my own, and leave the choice to the judgment of such, as shall consider impartially the one and the other deduction from the phænomena, which I take to be what concerns his own observations truly delivered by him. He relates then, (in his Preface) that he has three or four times visited the *Sudavean* coast, which is the principal place of *Prussia*, where the *amber* is found in the greatest plenty: and that he there did not only inform himself by what he saw, but by discoursing and examining the searcher, or fishers, for it, and the Overseers and Governors that took care of the whole affair, for the Prince's interest, that he collected, and carried away with him, not only pieces of *amber*, but several sands, clays, and other materials found with them, that he might be inform'd by judgment of others to whom he shew'd them, *&c.* This coast faces the west, and lies about 20 leagues N. E. by E. of the town of *Dantzick*. He adds, that it has been found also in many inland parts of *Prussia*, as well as upon other shores of the *Baltick* Sea; but thinks it to have been carried by the sea to such places from this shore. He mentions a piece found at *Gilyenburg*, 20 *German* miles from the sea, which was found in making a well, which proved to yield salt, not fresh water. Also at *Bortenstein*, a fountain breaking out brought with it much *amber*. And he mentions another two miles from *Bartenstein*, which in 1666. broke out in the same manner, and vomited, with the *amber*, a great quantity of sea-sand, which much damaged the fields; and it hollowed the mountain so much, that the top sunk in, and left a soundless abyss, or vorago. The *amber* thrown off was of divers colours, and bignesses; and there were various pieces of wood also mix'd with the sand: this efflux, at last, ceased; and it has now left a lake, and prodigous caverns. He says further, that digging a well at *Aschenburg*, they found *amber* in a bed of sand, like wood; but he thinks the wood to be clay,

shaped like wood. He mentions also many inland lakes where it is sometimes found, far distant from the sea. He mentions it found in making other wells; one in the year 1641, another in 1663, at whose bottom *amber* was found in beds of sea-sand. In other places trees were found also in the same sand. He relates many other places of *Prussia*, where, after the same manner, it has been found; and he could have instanced also in abundance more. This I find upon the whole, that it is almost all over *Prussia*; that it is generally found in a bed of sand; and, that other substances, as wood, iron, *&c.* are often found in that sand also. These substances, sand, amber, wood, trees, *&c.* he believes (p. 36.) to be the product of the sea; but to be convey'd thither by subterraneous passages: and this especially, for that planks, iron, and other parts of vessels, are found in the same sand of the inland lakes, and wells, where he thinks it impossible that there should ever have been any ships or vessels. He mentions it to be found in *Pomerania*, but in small quantities, and that only to have come from *Prussia*: describing further the places of the coasts, where 'tis found in most plenty, he says, the rocks and shore have many petrifacted stones, and that the clifts, or banks, are full of vitriol, or marquisite stones; and plenty of vitriol, niter, and other petrifying salts, are found mixed with the same sand, in which the pieces of *amber* are found (p. 51). Quantities also of thunderbolt stones, and *Prussian* diamonds, or chrystals, are also found with it. He proceeds in his 3d *Chapter* to describe the proper vein, or mine, of *amber*; and this, he says, no one has truly described besides himself. He says, there are three kinds of it; namely, a clayish, a woodish, and a sandy mineral; in one of which it is always found: the clayish is a sort of blue clay; the woodish consists of fossil wood, not vegetable, (as he thinks) but form'd out of the clayish one; some, he says, reject the clayish and sandy minerals, and think them to be the only true *minera* of *amber:* but he, by many arguments, endeavours to confute their opinion; especially, that of those who reject the sandy, because they could not conceive, how the sea-sand should be carried so far from the sea; which, he conceives, might be done by the universal deluge, or by the breaking out of fountains, like that which happened 1666. before-mentioned; or, which he

sticks to, that it has been convey'd from the sea by subterraneous caverns, which he thinks are now, and have been in time, all fill'd up by it, and so comes to be found all over *Prussia*.

But the other authors think the woody *minera*, to be the only and the true *minera* of *amber*; yet *Wigandus* thinks, that the places, where it is found, have been formerly covered and overflowed by the sea (p. 45). He grants, that the *Frisch Nerwing* has been so overflown, and is now firm land; but is not satisfied concerning other places, (p. 46). The woody vein at *Kraxtepellen* has much vitriol mix'd with the *amber*; and there is much niter also with the vitriol, (p. 49) and that almost every where, where *amber* is found, there is found much niter, as the miners do assert. He adds, that the sea does petrify substances into black stones, as he himself observed at the places where *amber* is found in most plenty, (p. 51). The diamonds are found in such petrified stones, when broken, like those I have formerly described in the hollow flints, (p. 52). A woody vein at *Gross Havenig* he survey'd, and found the hill to be all sandy, but the middle part was wood, like rotten trees, very black; they seem'd a kind of fir-trees, others thought them oaks; but he seems to slight what trees they may be like; for he will have them to be only clay, or earth, so shaped, (p. 6). But that at *Kraxtepellen*, he grants, was yet more plainly like wood, having nothing of earthiness mix'd with it. That which when moist was very black, when dried discovered more plainly its parts, and became of a reddish colour, (p. 61). In the cavities of these trees he found them fill'd with *amber*, and inclosed in the wood; yet he thinks the wood never was from trees; tho' yet he grants, that several of his friends and patrons assert them to be true wood. He adds, (p. 65) that they found them burn clear without mineral-stinking; but, he says, what he had found, stunk of niter mix'd with vitriol and sulphur: but this stink the alga burnt also yields, and stinks somewhat like garlick. He has much more about the *minera* of *amber*, &c. which I shall not trouble you with the epitome of, at present. I shall only acquaint you with what I collected by my observation of the whole, and that is, that all those parts, where the *amber* is found, as in beds, has been sometimes under the sea, and so has been raised from under it, as I have heretofore

made it probable that *England* has been; that it has been often tumbled with earthquakes, as *England*, has been where the fossil trees are found; that the trees have formerly grown where the banks are now found; that the gums of these, and such like trees, having dropped from them, have been, by rains, wash'd down into the river, and, by their streams, carried into the sea; that greater quantities have remain'd where the trees grew; and when, and where, they came to be thrown down, where they have remained, and since been petrified into *amber*, by the nitrous, vitriolate, and other saline substances, the products of saline eruptions; and that has been the true cause of the phænomena.

That such an exudation may be from trees, and that it may be so carried into the sea, I could produce many observations; but I shall only instance in one, at present, and that is, at *Bencoula*, on *Javaghen*, the *English* have a Fort, and factory, all their pitch, or rosin, is collected out of the stream of the river, or gathered on the banks and shores of the sea: and *Dampier*, in his *Voyages*, tells us, that the *Cochin China* men fetch their pitch from *Pulo Condore*, where, by cutting a notch in the bottom of the tree, it will run, every day, more than a quart of rosin each tree. As to the probability of petrifying of such rosins, I should say more, if I had assistance for making experiments, which at present is wanting. But I do not in the least doubt, but that the same thing may be perform'd by art, which is in this by nature. I could add many other arguments for this conjecture, from the smells of *amber*, from the things inclosed in it; as also some observations about *ambergreese*, and some other petrifactions; but for these I shall take another opportunity.

Dr. HOOK'S *second discourse of Amber.*

I acquainted you the last day, with what my author thought the most general and common *minera* of *amber*, which he conceives to be extended over all *Prussia*, as well in all the inland parts as in the sea, on the shores, and in the caverns of the clifts and hills out of which it is dug; and this he has confirmed by many particular instances, at some of which he has been a witness, and of others he has had very pertinent informations.

This is the *minera arenaria*, a certain layer, or bed of sand, which, by the substances found in it, does to me seem plainly to have been the bottom of some sea that has formerly covered all that country; which country has, in process of time, been rais'd above the level of the surface of the present sea; but yet, at a certain depth, all that sandy bottom yet remains, containing such substances as were there deposited whilst it was in that estate and condition; at least such of them as have not by length of time rotten and consumed. These more durable substances, I say, as the pyrites and petrified shells, which he calls thunder-bolts and wood, bones, and amber, together with the saline bodies of vitriol, alum, niter, and sea salt, are found to have been, to this day, preserved in it, and to be found unconsumed by the general devourer of all things, *time*. So that, when they have occasion of digging into this bed of sand for wells, or the like, or upon the accidental erruption of springs, lumps and pieces of *amber* are often found in it, together also with divers of the other permanent substances found commonly on the shores of the sea.

Now, that this is not so impossible or unusual a phenomenon, as should startle any one's assent, or belief of the truth of it, I did, 33 years since, prove, by multitudes of observations (divers made my self, and many more by others) that all *England* is a most evident instance and testimony of the like phænomena here; that is, that not only the vales, and lower parts of the land, have been sometimes the bottom of the sea, but even the tops of the hills and mountains, (such as we have) do plainly, and undeniably, confirm it. How, and when, these alterations have been effected, I have long since given my conjectures; but, if God restore my health, I hope I shall be able to give a more particular, convincing, and satisfactory account; not only founded upon the observations and phænomena I then had for my Directors, but many hundreds of others, which I have since that time collected; which have not only confirmed, in the general, what I then pitched upon, but has enabled me to be more particular in the mode, time, and method of them.

Now, if this phænomenon be thus solved, by granting that all *Prussia* has been formerly under the sea, and that this *minera arenaria* is a plain testimony of it; 'twill not be difficult to con-

ceive how the *amber* comes to be found in it, since the greatest part of what is now taken by those, whose business it is to find it, is by digging, and fishing it up out of the sand of the shore, or of those parts that are pretty near contiguous to it, and lie not very deep under the water; and these pieces of *amber* are not found on the top of this sand, but buried in, and covered by it, a pretty depth; not but that, questionless, the deeper parts of the bottom of the sea, if it were in the same manner digged and examined, would yield as great plenty of it; but I perceive they have not a method of making such experiments, and content themselves to fish for it only in the shallower parts, and on the shore. But still the question is, how, and from whence came it, and by what means to be there placed? That then is the next enquiry.

And here, for the answering of this, we must *audire alteram partem*, that is, the judgment of those which he acknowledges to have been the principal who have treated of this subject, and those from whom (besides his own observations) he hath collected the chiefest of his informations, whom he calls *Triga eruditorum Prussiæ*, i. e. *Aurifaber, Gobelius, & Wigandus, viri de Succini notitia optime meriti:* but, tho' he praiseth these, yet he quotes, and makes use of the relations and testimonies of many others also. But yet, as to the true *minera*, or vein, or proper scent of it, he rejects the opinion of them all, and endeavours, by his whole discourse, to confirm his own opinion; which he calls his own, because, says he, (p. 55.) *Hic locus quidem (quantum scio) diserte a nemine explicatus.* And yet, (he adds) *Proprias autem venas ut aliorum mineralium ita etiam & succinorum extare, tam certum mihi quam quod certissimum.* (We must allow him some grains for his fondness of his own opinion) *Neque solum id confirmat quod peculiaris signorum cognitio in fossoribus requiratur, ut quæ propter singularis curæ venas indagandi & observandi cuipiam in angulo ad* Gross Hubenig *ubi præprimis foditur, es demandata, sed quod hujus ætatis eruditi Physici Chymici qui illa loca adierunt, aut terras inde allatas fuerunt accuratius contemplati, itidem venas mecum flatuant, sed & sequentia assertum nostrum manifestum reddent, ubi etiam per totam Prussiam si qua altius ex terra effossa, signa venarum adfuisse constiterit.* I shall not trouble you with the relation of these *sequentia*, but shall only

say, that the hypothesis I have mentioned, of the whole country's having been sometimes overflowed by the sea, does give a full solution, and explication, of them all; and, indeed, they are, most of them, very confirming proofs of that doctrine, if they be duly considered; as I could shew, if it were not too tedious: for, how should the broken pieces of the pitched plank of a ship otherwise come to be found in his *minera arenaria*, or *minera lutea*, at so great a distance from the sea. He grants, indeed, that the *amber* found on the shore, in the sand, is not there in its proper *minera*; but it is by accident thrown up by the working of the sea, and, by the same cause, covered and buried in the sand: but, when it is found in the inland parts, then he thinks it to be in its proper *minera*. *Alii arenosam & luteam negant, & casu vel forte immista succina asserunt, unam ligneam genuinam venam autumantes. Verum arenosam ut ut illa probarent loca quæ ex Prussia & Pomerania dedimus, quæ scilicet arenis obtutum succinum dedere.* Again, *De collibus vero & montibus arenosis idem asserendum*) (that is, that the *amber* has been accidentally, or by the working of the sea, mixed and buried in the sand), *difficilior est ratio, imprimis quod a mari satis sint remoti. Quis vero casus his vel fingi potest succina & quidem non contemnenda copia credidisse? An ad inundationes terrarum recurrendum? Sed illas nondum ubique historica fides satis adstruxit. Potius, ut quæ mea sit sententia exprimam, meatibus subterraneis eadem deberi contendo, & cum scaturigine aliquando ejecta fuisse non aliter quam ad* Bartenstein *Anno* 1666. *contigisse recensuimus, & hic multum arenæ simul egestum, & credo sub illa etiamnum latere succina.* So that we see he is forced, tho' unwillingly, to yield, that 'tis possible the *minera arenaria* may be a product of the sea; tho', because he finds no history when the country was overflowed by the sea, he would evade that way, and introduce his notion of subterraneous passages; which is, as if a mariner discovering an island in some great ocean, and finding some house on it, but no inhabitants, should conclude that this house had there grown of itself, or else had been brought thither thro' the air by some violent hurricane, and there set down, (for I fancy a hurricane might as easily carry a house threescore miles thro' the air, as subterraneous passages convey the sand, and *amber*, of the sea-shore, to a mountain

threescore miles in the land), and he should make this conclusion, because he wanted a history of the habitation of this island by some men. But (as I said before) we must allow the author some grains for his kindness to his own off-spring. But, as we have hitherto made him some grains of allowance for his partiality for his hypothesis of the *minera arenaria,* and the subterraneous conveyances, where he is forced to yield it may be sea-sand; so we must now allow him some drachms, or rather ounces, where he would evade the *minera lignea* of *amber*; for this *minera* seems to spoil his *arenaria*: for, tho' almost all the other authors do make this to be the chiefest, and most natural *minera,* which affords, by much, the greatest quantity, and the biggest, and most entire pieces; and tho' he agrees with them, by his own experience and observation, yet, since it would depose his *minera arenaria* from the first dignity, by one salvo he evades all the stress of it against his *minera arenaria,* by making it but one species of the *minera lutea*: for, he would have the wood that is found, not ever to have been trees, but only clay so shaped, by he knows not what cause. For he says, (p. 61.) after he has told the several opinions of divers authors concerning the species of the trees that compose the *minera lignea, verum parum interest scire cujus ligni præferat faciem, cum genuinum lignum non esse in physicis demonstretur satis.* And so again, after he has more particularly examined the words and assertions of the most celebrated authors, concerning this opinion, and opposed them, as much as he was able, (which answers, to me, I confess, seem very insignificant, and, at best, but evasions) he says, (p. 182.) *quare cum nec historia nec rationibus solide probari possit, succinum arborum esse succum, parum interest discrimen lachrymæ, gummi, & resinæ, hoc loco annotare & disquirere quo nomine convenientius succinum fuerit appellatum.* But, notwithstanding this, what he himself has observed and related, concerning this *vena lignea*, seems as great an argument against his own opinion, as any can be brought. He says then of his own observations (p. 59.) *diversimode contemplari contigit ad Craxtepellem totum montis jugum contectum quasi corticibus grisei coloris vidi. Superiorem enim faciem Soli expositam ita calor exsiccaret: remota vero hac parte extima piceæ nigredinis terra magnis quasi & levibus nitidisque*

crustis persitus concreta conspiciebatur; atque si cultro dissecabatur, quasi multos mollissimos cortices dissecuisses, secta præ se ferebat introrsum versus vero soliditas compacta terra difficilem sectionem reddebat.

This is his first observation, which, how plainly it describes trees, I leave any one, unprejudiced, to judge. First, they were found at the top of the mountain, where, in probability, they had grown; and where, by the way, 'tis not very likely that there should arise a Fountain of *Bitumen*, or that the *amber* should be conveyed thither by subterraneous passages; and yet plenty is there dug out: and whilst he was there, he says, there was taken up *unum ghetalum succini*: what *ghetalum* signifies I know not. Next, how proper his opening, or dissecting of this ground, as he calls it, does represent a rotten tree, you may easily judge: for, first, he describes the substance of the bark, or rind; next, the sappy parts of the rotted tree; and, lastly, the heart, or solid woody part of it.

His second observation is this, (p. 60.) *Aliter ad Gross Hubenig venam ligneam cum fossorum operis conspicere datum fuit. Mons erat arenosus, plane intermedium erat genis legni quod putredo emolivisse videbatur, ut facillimo negotio bipalio instar mollissimæ terræ a fossoribus radi posset, nigro quasi carbonis colore infectum; specie abiegno non absimile, imprimis cum in ejusmodi cortices circulares veluti deglubi poterat, & alias ejusmodi intersegmenta, sive lineamenta ostendebat: alii querno comparant, sicuti & frusto ligni* Spork, *scilicet fragmenta quæ cum succino ejici postea dicendum erit, ejusdem generis credunt.* Read the book, page 61, 62, 63, 64, 65, and 66, to §. viii.

That some of these pieces of gum have been found not quite petrified, but only so far as to have some degrees of it, yet to be mouldable like wax; further, that the country has been sometime overflow'd, and that of the remainders of the sea have been left in several parts of the country: but, besides the sea-water, it seems to me, by several passages in this book, that I could quote, that the land of *Prussia* abounds with these kinds of petrifying substances, rather than that that country was the only place where those kind of trees grew; and, that it seems by the differences of *ambers*, found in very distant parts of the

earth, that other sorts of resinous gums may be turn'd into *amber*, if the petrifying substances be afforded, where such gums do drop from their proper trees: now what is the true petrifying substance of *amber*, I have not observations enough to determine, nor have I wherewith to defray the charge of experiments for that purpose. Some conjectures I have, concerning other kinds of petrifications, for there are many kinds of that operation, which I may, some other time, discourse of, and, if I have conveniency, shew some experiments about it: 'tis a subject that deserves to be cultivated, for it will afford very much of information in physical productions, and 'tis, I conceive, much differing from the sentiments of authors I have hitherto met with, who have treated of it. But I fear, I have been too tedious on this subject, and therefore shall say no more at the present, only I shall shew a specimen or two of another sort of petrification, and those are of chalk, which though from its plenty, it be more vile, yet, for that very cause, it seems to me to be well worthy a more serious and diligent enquiry, to find out from what substance that body had its first original, for by the instances that I shall shew, it appears plainly, that it was a fluid body before it became a solid; and by other instances also, it appears, that flints were likewise so before they were petrified into flints, and so several other stony concretions, of which subjects, little is to be found in natural historians.

1697

May 19.

A third discourse of Dr. Hook's *concerning Amber.*

Since I read some discourses here the last Vacation, concerning my conjecture about the original of *amber*, in which I endeavour'd, by many arguments, to prove it to be a petrifaction of a vegetable juice, or the resinous gum of some tree, I had occasion to search into the *Acta Hafniensia* of *Thomas Bartholine*, for another enquiry, and so accidentally met with some curious observations of that learned man, concerning this subject of *Amber*; some of which I conceive, if not all of them, do much contribute to establish the doctrine, or opinion, which I endeavour'd to maintain.

That which I principally took notice of is, the 57th Head or Section of the first Volume, for the years 1671, and 1672. Published at *Copenhagen*, in the year 1673. It contains an account of observations and experiments about *Amber*; where, first, he relates, that the diggers of the new ditch, about the city of *Copenhagen*, met with pieces of *amber* of several bignesses; and, which was very remarkable, the diggers took notice, that wherever they found these pieces, they found them mix'd with the *minera* of *amber*, namely, the bark or rinds of oak-trees, with which it was not only mixed, but stuck, or glued fast to it, as is to be seen, says he, in the several pieces which the diggers have sold to divers curious persons. There was also another mineral, which was a black wood, as if burnt, to which the *amber* also stuck. I should, says he, have believed it to be some sort of bitumen, or black *amber*, if the smell of it had not made me of another mind; for the ill smell of it, when burnt, made me judge it to be the remainders of some pieces of oak. And yet *Camden*, says he, in his description of *Whitby*, mentions such a black *amber*, or jet, to be found in *England*. The passage in *Camden* is this; speaking of the parts near *Whitby*, in the North-Riding of *Yorkshire*, he says, *Juxta hunc locum & alibi in hoc littore repertum est Succinum nigrum sive Geate, Gagatum aliqui esse existimant, quem inter rariores lapides gemmasq; habuerunt veteres. Enascitur vero inter cautes ubi rimis debiscunt; & priusquam expoliatur, colore est subrufo, & æruginoso. Expolitum autem vere est, ut inquit* Solinus, *Nigro-Gemmeus, de quo Rhemnius Palemon è Dionysio.*

—— Præfulget nigro splendore Gagates
Hic lapis ardescens austro perfusus aquarum,
Ast oleo perdens flammas, mirabile visu,
Attritus rapit hic teneras, ceu succina frondes,

Et *Marbodæus* in suo de Gemmis Libello;

Nascitur in Lycia lapis & prope Gemma Gagates,
Sed genus eximium fæcunda Britannia *mittit;*
Lucidus & niger, est levis & lævissimus idem:
Vicinas paleas trahit attritu calefactus,
Ardet aqua lotus, restinguitur unctus olivo.

Audi etiam solinum. *Gagates in* Britannia *plurimus optimusq; lapis; si colorem requiras, Nigro-gemmeus; si qualitatem, nullius fere ponderis; si naturam, aqua ardet, restinguitur oleo; si potestatem, attritu calefactus, applicita detinet.* Thus far *Camden*; from all which to me it seems very probable, that the true jet is a kind of *amber*, and differs from the common yellow *amber* only in its colour, which is very black; but 'tis found, as the other *amber* generally is, only in small pieces, most commonly in the clefts of stones, and which is further remarkable, where there are also found several other substances, preserved by petrifaction; for just before this passage, about black *amber*, in the same page 485 of my edition, he, mentioning other remarkables of the same place, says, *Lapides hic inveniuntur, serpentium in spiram revolutorum effigie, naturæ ludentis miracula,* (*quæ ut inquit ille*) (he means *Bede*) *natura cum veris & seriis negotiis quasi fatigata ludendo efformat. Serpentes olim fuisse crederes quos lapidens cortex intexisset. Hildæ autem precibus adscribit credulitas, tanquam illa commutasset,* &c. I suppose he means the *Cornu-Ammonis* stone, of which kind, many are found in *Yorkshire* by several, but more particularly by Sir *Jonas More,* who assured me, he had seen one, and knew where to fetch it, which was full as big as the fore-wheel of a coach, which he promised to get, and convey to *London,* whenever he went into that country; and that there was great plenty of others of somewhat smaller sizes, yet of the bigger kind; divers of which kind are in the repository, though found in other parts, as particularly in the quarries of *Portland,* and at *Keynsham* in *Somersetshire,* by Mr. *Waller*; nor are these kind of petrifactions in *Yorkshire* only about *Whitby,* but multitudes also are found in *Richmondshire,* as the same author, Mr. *Camden,* testifies, (page *mihi* 489.) *Incisis rupibus & montosa collium eminentia hæc regio fere tota eminet quorum convexa sunt alicubi, sunt satis herbida,* &c. *Montes plumbo, carbone fossili, necnon ære gravidi,* &c.——*Quod in eorum autem summitatibus ut etiam alibi, lapides nonnunquam fuerint reperti, cochleas marinas & alia aquatilia referentes, si non sint naturæ miracula: refusi in omnem terram sub Noe diluvii certa esse indicia cum Orosio Christiano historico judicabo. Sic enim ille scribit,* &c. But to pass this by, which I have only taken notice of, to shew, that about

those parts there are sufficient indications of petrifactions of other substances also; and thence we have the more reason to conclude, that *amber* also, both white, yellow, and black, are petrifactions also, and that the colour may proceed, either from the particular nature of the original gum, or else from the differing sorts of the petrifying mineral salts; for 'tis sufficiently known, that oak turns to black with a vitriolate mineral, and to red with an aluminous; or that the black may have been produced by the effects of a subterraneous fire there having broke forth, as pitch and tar are strain'd by the power of fire, in the artificial making them, by burning of the wood, out of which they are forced; and as the vitrious jet, of which we had formerly some specimens here, presented by Sir *Robert Moray*, which were brought from the burning Mount *Hecla* in *Island*; which black substance was a perfect glass, and, by melting of it in the strong flame of a lamp, I reduced several pieces of it to clear transparent glass, the thickness thereof vanishing, by the keeping it for some time melted in the hot flame of the lamp: but however this black *amber*, or jet, comes to receive such a tincture, it seems plainly to me, to be of the same nature with yellow *amber*, and both of them very different from those substances that are originally mineral, as *asphaltum*, or other bituminous substances, especially by their lightness and fineness of texture, as their artificial polish does plainly manifest. And *Bartholine* seems plainly to be convinced of the truth of this hypothesis by many passages, related in this 57th observation; as particularly, that it has been left where it was found at *Copenhagen*, by the sea; and that all that country has sometimes, formerly, been overflowed by the sea. Next, that all *amber* has been first soft, and, by process of time, indurated; that, when soft, it was the gum of some tree; and, while so, those several substances were immers'd in it, which afterwards became cased up, and inclosed in the same substance hardened, or petrified; as, *Joh. Gobelius* had a green frog so inclosed, and *Frederick* III, King of *Denmark*, had a lizard after the same manner: and Mons. *Picart* was presented, by *Scholerus*, with the cone of a fir-tree inclosed in the same manner. *Non igitur dubitamus*, says *Bartholine*, *liquidam fuisse resinam vel Lachrymam ex arbore prostuentem, & vel sale, vel temporis diu-*

turnitate in maris littoribus concrescere & indurari: quanquam probabili ratione quoque, alii ex pingui bitumine in istam soliditatem compingi suspicentur. As to his other trials about the dissolution of *amber*, mentioned in this 57th *Section*, I omit them, as affording little of information pertinent to the solution of this query, whether it owes its original to a vegetable or mineral substance? And pass on to the 122d observation of his second tome; where, upon the occasion of some objections made against his supposition, by *Joh. Dan. Major*, Professor in the University of *Kilee*, he has enumerated all the observations which he conceives to be pertinent to the determining this controversy.

1. The cone of a fir-tree included in *amber*, my friend *Sextus Scholerus*, Consul of *Copenhagen*, had.

2. I saw, at Mr. *Henry Monachen's*, my honoured kinsman's, a piece of *amber*, composed of white, yellow, and green parts, in which was included a gnat, and some of the moss of a tree.

3. *Wigandus*, in his history of the *Prussia amber*, relates, that he saw a green frog, which is used to sit on the green leaves of trees, included in a mass of *Prussia amber*.

4. The sticking of gumlac to its sticks gives a suspicion that *amber* may stick in the same manner; tho', being liquid at first, it may not stick to the twigs, but drop down from them.

5. That most gums, which flow out of trees, do not carry with them the impression of those trees.

6. That those small creatures, as flies and gnats, which are found in *amber*, do pitch on such parts of trees where the gum trickles down, and so are as likely there intangled in it, as in the earth; where they do not only abscond, during the winter months, benummed as 'twere, and half dead.

7. If you believe *Tacitus*, birds also have been found in *amber*, whose words, in the book of the manners of the *Germans*, are remarkable, and not disbeliev'd by any. *Succum tamen arborum esse intelligas* (says he) *quia terrena quædam atque etiam volucria animalia plerumque interlucent, quæ implicita humore mox durescente materia eluduntur. Fæcundiora igitur nemora lucosque, sicut Orientis secretis, ubi Thura Balsamaque sudantur, ita Occidentis insulis terrisque inesse crediderim, quæ vicini solis radiis expressa*

atque liquentia in proximum mare labuntur, ac vi tempestatum in adversa littora exundant. Si naturam succini admoto igne tentes, in modum tedæ accenditur, alitque flammam pinguem & olentem; mox ut in picem resinamve lentescit: thus far *Tavitus*. Now, says *Bartholine*, if this account be true, why should we doubt the former arguments; especially, since the natural historians, *Solinus*, and others, agree with him: nor is the fidelity of *Olaus Magnus* to be wholly rejected, tho' he had dreamt in some things.

8. The barks of trees are always found mingled with the *amber*, where-ever it has been dug up with us.

9. The feathers of birds have not been observed in *amber*; because the bird sits on the branches, and not against the body of the tree, where the gnats, flies, and other small insects do creep.

10. In *Norway*, where the pines, and other resinous trees abound, there are found lumps of gums emulating *amber*. The inhabitants call it a stone, and my honoured kinsman, *Joh. Finchius*, brought hither one of those lumps, which was a kind of *amber*; for it seem'd to be a light stone, or a black sort of horn, which would kindle, and burn with flame; but it stunk much: otherwise it seemed a kind of *lignum fossile;* yet it did neither burn so readily, nor stink so much, as black *amber*.

11. As to *ambergreese*, which is brought from *Florida*, tho' it be doubted by me, whether it be made of the sperm of a whale, or the semen of an elephant, as *Ctesias* is said, by *Aristotle* in the 2nd Book of the *Generation of Animals*, to assert; or of the dung of certain birds of the *Maldives*, which feed on odoriferous plants, as *Ferdinando Lopez* conceives; or a composition of lignum, aloes, civet, storax, and laudanum, as *Fuchsius* supposes; or a kind of bitumen ouzing out of the bottom of the ocean, as *Guliel. Du Val*, in his *Phytologia* asserts it; yet, I dare affirm, that it has the same original as yellow *amber*: for, there has been lately found some of it in *Prussia*; and, I cannot doubt, that there may be trees found in the New World, yielding odoriferous gums. Thus far *Bartholine*.

To whose arguments I have only six of my own to add, which seem to me as convincing, if not more, than all these. And those are,

1. That it appears, by all the relations we have of the finding of the yellow, black, or gray *amber*, that they are never found in any very large pieces; but only in such lumps or pieces, as may very well be supposed the exudation of a gum out of one or two vents of the same tree. Whereas, were they mineral, I see no reason why they should not be found in as great masses as asphaltum, canall, scots-coal, or bitumen, are usually found.

2. That all kinds of *amber*, of whatever colour, whether white, yellow, green, or black, are very light, and almost of the same weight with water, being but $\frac{1}{12}$ part heavier; so that it will but just sink: whereas those other substances, as canal, or scots-coal, are very heavy generally, and more than double the weight of water.

3. None of these substances do seem to have any peculiar figure, as to be formed into plated or prismatical bodies, as those substances I last mentioned have, especially such as have transparency, as talk, selenites, chrystals, *&c.* and the uniformity, or continuity, of the mass, plainly proves, that it was perfectly united, whilst yet fluid, and not form'd by chrystallization, or concretion, as salts out of brines, or sugar-candy out of syrups; or petrified spars, or chrystal, out of sea-water.

4. That turpentine, by being buried in the earth, for some considerable time, will yield, upon distillation, an oil perfectly resembling oil of *amber*, for colour, and smell, as was above 30 years since proved by Dr. *Daniel Cox.*

5. That there is no other mineral substance that is so light and rarified as this, which will take and receive so curious a gloss, and polish, as this will receive; whereas, of vegetable substances, we have instances enough in hardened gums, *&c.*

6. That there are instances enough to be found of the petrifaction of vegetable substances; and so this cannot be look'd on as a singularity in the parts.

These, I confess, to me seem to be *experimenta crucis*, as the Lord *Verulam* says; and I very much doubt, whether there can be any one argument as convincing, as each of these, for the contrary opinion. However, I leave every one to judge of both as he shall see most reasonable, and propound these arguments only, as those which have inclin'd me to be of this opinion.

The weight of a piece of *amber* in the air is, —— 2443 *grs.*
And in water —— —— —— —— —— 202.
And is to water near as $1\frac{82}{1000}$ or $\frac{8}{100}$ parts.
Amber to water is as 12 to 11.

$$\begin{array}{r} 2443 \ (\frac{102}{2241} \\ 202 \ \text{——} \\ \\ 2241 \end{array}$$

1697

May 26. Lecture on *Animal Substances* and of a Ship with the bodies of 40 men *buried in the Ground.* An account of a *Roman Ship* at the bottom of *Lake Nemi,* supposed to be there ever since Tiberius's time. [*Posthumous Works,* p. 438.]

July 31. Extract from the Transactions of the Royal Academy of Sciences touching the *Analysis of Infinites.* [R. S. MS., No. 90.]

Dec. 8. *On* M. HAUTEFEUILLE'S *method of shorter Telescopes.* [R. S. MS., Nos. 91, 92.]

n. d. Of the Refraction of the Atmosphere. [R. S. MS., No. 93.]

1697/8

Feb. 5. *Letter from* R. KNOX *to* HOOKE. [MS. *Sloane* 1039, f. 111.]

1698/9

Feb. 14.

Observations concerning the refractions of the Atmosphere.[1]
[Derham, p. 338.]

The doctrine of refractions does so sensibly affect almost all astronomical observations, that, till that be well establish'd, these will be too weak to support the conclusions which are generally inferr'd from them. At present, this doctrine is involv'd in this one great uncertainty, *viz.* the air being no uniform fluid, the rays of light are not refracted in any one terminated superficies, but continually into a curve; and it is not easy (if possible) to determine the nature of that curve, till we know the propor-

[1] These observations, I conceive, were the Reverend Mr. *Lowthorp*'s, being written in his hand. They bear date *February* 14, 1698–9, and precede the experiment he made at the request of the *Royal Society* the month following, March 28, 1699. Of which an account is given in *Phil. Transact.* No. 257. W. DERHAM.

tion of the powers of refraction in the several densities of the atmosphere.

That the attempts, hitherto made by astronomers, are not satisfactory, I think, will be allow'd, when it is consider'd, that, if (according to the receiv'd opinion) the distance of the moon be about 60 or 61 semidiameters of the earth, and the horizontal refraction above 30′, the moon at an eclipse passes thro' the focus of the atmosphere, or very near it; and that every distinct point of the moon's hemisphere is illuminated (even in the middle of a central eclipse) by rays flowing from every point of the sun's hemisphere, which is directly contrary to the nature of an eclipse. We seem, therefore, under a necessity, either to remove the moon in the Planetary System above 20 semidiameters nearer to the earth, that it may fall into that part of its shadow, which the duration of central eclipses require; or to form a new theory of the refractions of the atmosphere. I am sure the first would so far confound our receiv'd astronomy, that he would be a very bold man who durst venture to maintain such a paradox: but I hope the proposal of the following experiment, relating to the latter, will be excused; because it may, perhaps, be of use towards the removing this great doubt.

Upon an air pump place a small receiver of copper, having, on each side, an even, well-polish'd, flat glass, and moderately thin: let their angle of inclination to each other be about 65 degrees, *viz.* with a telescope, thro' these glasses, whilst the receiver is full of air, a thread placed at least 40 foot from them; and, as the pump reduces the air to several degrees of rarity, (which may be measured by a barometer inserted into an end of the receiver) remove the thread, till it appear in the same place in the focus of the telescope, as at first. By this means the angles of refraction, and incidence, may be easily found, and more certainly determin'd than any other way yet publick. And if this experiment be repeated in several temperatures of the air, I doubt not but such a theory of refractions may be establish'd, as may be depended upon, to confirm, or reform, astronomy.

Perhaps this experiment may be made, more conveniently, by filling the receiver with quicksilver, and pumping it out; which will leave the receiver absolutely void of air.

This experiment must be made with great nicety and exactness: for, according to the common tables of refraction, this inclination of the glasses to each other (one of them being at right angles to the axis of the telescope, and may be its object glass) will not produce, for the angle of refraction, above 4′.

The charge will not be above two guineas, or two and an half, if made with quicksilver; and the materials will be worth most part of that money again, whenever disposed of: but, if the air pump can be so fix'd, as not to shake, or change its situation with working, the charge will be very little.

Let *a b c d* be the superficies of the earth, and *e f g h i k* of the air, having a common center *C*. Then suppose *e g i m* to be a cylinder of light flowing from a small part of the sun, equal to the earth, and the extreme rays *l g* and *m i* refracted (by their immersion into the air) towards the perpendiculars γ *C* and δ *C* becoming thereby horizontal at *b* and *d*; and by their emersion out of air, from the perpendiculars ϕ *C* and κ *C*, and to intersect and the axis of the cylinder of light at the focus F. Let the angles of horizontal refraction, towards the perpendicular λ *g l*, and μ *i m* be 30′, then the angles of horizontal refraction, towards the perpendiculars α *f F*, and β *k F*, will be also 30′, and then the angles *f F C* and *k F C*, will be 60′, or 1°. And then the semidiameter of the Earth *b C*, subtending that angle (and near) is near $\frac{1}{60}$ part of the distance of the focus *F C*, and therefore the Moon's place at an Eclipse.

Fig. II. Let *a b c d* be the superficies of the Earth, and let the concentric circles 1, 2, 3, 4, 5, 6, 7, and 8, 9, 10, 11, 12, and 13, 14, 15, 16, and *e f g h i k l m n o*, be supposed so many distinct densities of the air, and *L L* the diameter of the Sun. If the rays *S i* and *S l*, flowing from the center of the Sun *S*, be refracted by their immersion into air, towards the perpendiculars *t C* and *u C*, and become horizontal at *b* and *d*; and be refracted by their emersion from the perpendiculars *p C* and *z C*, and intersect each other at *M*; then the parallax of the Sun *i S C* (suppose) 48″, being subtracted from the horizontal refraction, by immersion σ *i S* = 30′, and the remaining 29′ 12″ being added to the horizontal refraction, by emersion ϕ *f M* = 30′, the sum is the angle *f M C* = 59′ 12″; therefore the semidiameter of the Earth,

$b\,C$, subtending this angle (and near at right angles to $M\,C$) is more than $\frac{1}{81}$ part of the distance of this intersection, and therefore not far from the Moon's place in the middle of a Central Eclipse.

From hence 'tis obvious, that, if the rarer air have a less power of refraction, the rays $L\,R$ and $L\,m$, flowing from any parts of the Sun's hemisphere, (as L and L) may fall upon some part of the atmosphere, where the angles of refraction $\lambda\,h\,L$, and $\gamma\,g\,M$ (being less $\sigma\,i\,S$) will direct it to the same point M. Therefore M is illuminated by rays flowing from every part of the Sun's hemisphere: and therefore, if the Moon be at this distance, every distinct part of its hemisphere will be illuminated by rays flowing from every part of the Sun's hemisphere.

I am in too much haste to be exact, either in the exceptions, or reasonings, but I hope thus much will sufficiently appear, that it is very difficult to account for the phases of the Moon upon the receiv'd hypothesis, and that further satisfaction is to be wish'd, which is all the use I design'd to make of them.

Dr. HOOK'S *answer to some particular claims of* Mons. CASSINI'S, *in his Original and Progress of Astronomy.* [Derham, p. 388.]

Having lately perused a discourse of Mons. *Cassini*, concerning the original and progress of astronomy, and of its use in geography and navigation, I could not chuse but take notice of several passages of it, which seem more particularly to concern this *Honourable Society*; and the rather, because I do not find that it hath been mentioned by any hitherto, but suffered to pass into the World for authentick, and will be so concluded by the future learned World, if it be not otherwise informed of the errors, or mistakes, therein contained.

The first is, concerning the beginning, and original, of the *Royal Society*: concerning which he might have been much better informed, if he had taken notice of what is said concerning it in Dr. *Sprat's* History thereof; but that, it seems, did not so well suit to his design of making the *French* to be the first. He makes, then, Mr. *Oldenburg* to have been the instrument, who inspired the *English* with a desire to imitate the *French*, in having Philosophical Clubs or Meetings; and that this was the occasion of founding the *Royal Society*, and making the *French* the first.

I will not say, that Mr. *Oldenburg* did rather inspire the *French* to follow the *English*, or, at least, did help them, and hinder us. But 'tis well known who were the principal men that began and promoted that design, both in this City, and in *Oxford*; and that a long while before Mr. *Oldenburg* came into *England*. And not only these Philosophick Meetings, were before Mr. *Oldenburg* came from *Paris*; but the Society itself was begun, before he came hither; and those, who then knew Mr. *Oldenburg*, understood well enough, how little he himself knew of philosophick matters.

The next thing, I take notice of, is his asserting the *Royal Academy*, at *Paris*, to be the inventors of many inventions, and improvements, of astronomical helps, which were invented, and improved here, by some of this *Society*, before that at *Paris*, was founded.

The first thing, he instances in, is the pendulum clock, which, he says, was invented by one of the members of that *Academy*. I suppose he means Mons. *Chr. Huygens*, because he mentions the regulation of them by the cycloid: now, 'tis well known, that this person was a member of the *Royal Society* four or five years before the *Royal Academy* was founded, which was not till the year 1666: the *Royal Society* has, therefore, more right of claim to that improvement, than the *Royal Academy*; but, indeed, the invention was precedent to both, and was made in *Holland*, and from thence sent into *England* about the year 1659, or 1660.

The next thing, he lays claim to, is the regulation of watches, by a spring applied to the balance; but that is somewhat more injurious than the former: for, it was not pretended to by Mons. *Zulichem*, till about the year 1675; whereas it was here invented, before the year 1660; in which year, I, and three other members of this *Society*, had a grant of a Patent for the use thereof; and some years after, when Mons. *Zulichem* came to be informed of it, he wrote a letter against it as a thing not practicable.

The 3rd thing is about the finding a standard for an universal measure by the length of a pendulum vibrating a certain time. This, I believe, was first invented, and tried, by Sir *Christopher Wren*, some years before the beginning of the *Society*.

But that this length would not be the same, all over the World,

was discovered by me to this *Society*, 32 or 33 years since, as will appear by the Registers of this *Society*.

The 4th thing, he instances in, is the improvement of telescopes, both for length and goodness, which was first performed here by Sir *Paul Neile*, Sir *Christopher Wren*, and Dr. *Goddard*, who instructed and employed Mr. *Reives* in the manual operation; and, by that means, it was carried to the perfection of making object-glasses of 60 and 70 foot long, very good, before any mention was made of such being made in *France*. Some such attempts, indeed, had been made in *Italy*, by *Divini* and *Campani*: but upon comparing one of the best of them, brought hither by Mr. *Monconys*, I found, that a telescope I had then by me, of Mr. *Reive*'s making, of the same length with the *Italian*, was full as good, if not better; which Mr. *Monconys* acknowledged.

A 5th thing, he instances in, was a way of using these object-glasses without tubes. This I practised here long before any mention was made of its being known beyond sea, where, I suppose, it was first used by Mr. *Huygens*, who hath printed a little discourse concerning it; but that was above 20 years after I had used it here in *England*.

A 6th thing is the application of clock-work, to keep the glass directed to the object; but who contrived this application, will appear by my animadversions on the *Machina Cælestis* of *Hevelius*.

A 7th thing, he instances in, is the application of telescope sights to instruments, which was invented and perfected here long before any such were to be found, or heard of, in *France*. And Mr. *Bullialdus*, and several other of the *French* astronomers, as well as *Hevelius* in *Dantzick*, and Dr. *Wallis* here, did disapprove of them, after I had published the use and great benefit of them, for sights of instruments, in my Micrography, in my attempt to prove the parallax of the Earth's orbit, and in my animadversions; and by the letters published by *Olhof* for *Hevelius*, it will appear how much the World was then of another mind.

An 8th thing is the use of a micrometer, &c.[1]

[1] Concerning which I shall refer to our *Philos. Transact.* No. 352, where I have given a sufficient answer to his claim of the *French* gentlemen, by asserting that and other inventions to Mr. *Gascoigne*. W. Derham.

It would be too tedious to mention all the particulars, which he intitles the *Royal Academy* to the honour of the invention of, to which, in truth, they have no just pretence of claim. However, I conceive, it might not be improper for some person to vindicate the right and just claim of this *Society*, that may stop the mouths of some malicious men, who will needs say, that this *Society* hath invented or improved nothing of real use.

1699

May.

A paper 'concerning Mr. Hooke's work'.

[R. S. MS., No. 94.]

July 30.

A discourse of the Causes of Earthquakes.

[*Posthumous Works*, pp. 424–8.]

Nov. ?.

Note on the *Batavian Earthquake of 8 Feb.* and R. PRIOR's letter on the *ridge of Mary Burrow* in Ireland.

[*Posthumous Works*, pp. 437–8.]

1701

February.

(41) *An account of Dr. Robert Hook's Invention of the Marine Barometer, with its Description and Uses.* By E. Halley.

Phil. Trans., p. 791, fig. 8, February 1700–1.

"These instruments are made according to the direction of Dr. HOOK, by Mr. HENRY HUNT, Operator to the Royal Society, who will furnish any Gentleman with them, and give them directions how to use them."

1702

June 2.

Letter from THO. CROWLEY *to* HOOKE. [MS. *Sloane* 1039, f. 128.]

1702–3

March 3.

On this day ROBERT HOOKE died, aged 67 years 7 months.

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